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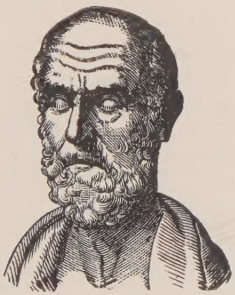

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BULLETIN

OF THE

NEW YORK ACADEMY OF MEDICINE.

STATED MEETING, FEBRUARY 21, 1866. DR. JAMES ANDERSON, PRESIDENT, IN THE CHAIR.

DISCUSSION ON EPIDEMIC CHOLERA.

DR. METCALFE, by appointment, opened the discussion as follows:—

MR. PRESIDENT:—Fellows of the Academy have been notified that the consideration of Asiatic or Epidemic Cholera would occupy their attention this evening.

At the request of the President, I shall make a few remarks introducing the subject to your notice; not in the way of a didactic discourse, but merely by mentioning, as succinctly as shall be in my power, such generally received facts as past experience has furnished to medical science. The literature of this malady is so copious, and in some respects so perplexing, that I could not, profitably, do more.

There is a peculiar propriety in considering this disease by us at this time. It is true that we can say nothing with positive assurance concerning the future; but I am confident that I do not err in saying that an epidemic visitation of our dreaded antagonist is to be reasonably looked for between now and the close of summer. "Its approach is discerned afar off as distinctly as a storm is foreseen by the rising of the clouds from the horizon, in the direction of the wind." The storm *may* pass round us or burst before reaching our homes. None the less is it our duty to act in such a manner as shall enable us to avert the calamity if possible, or to diminish its dangers if the advent be inevitable. The profession and the public have a right to expect this at our hands.

With regard to the diagnosis of cholera during an epidemic visitation, there is very little difficulty. We know of no other malady in which a number of cases occurring without ingestion of some known poisonous substance, present the following symptoms: Copious discharges, by vomiting and purging, of turbid, serous fluid; cramps, coldness of the body and of the breath; the pecu-

liar facies and aphonia; severe abdominal pains; unquenchable thirst; suppressed urine, dyspnœa, and collapse; purple or blue color of the skin, with occasional erythematous eruption; pulselessness; shrinking of the body with inconceivable rapidity; shrivelled, insensible, and inelastic skin; apathetic state of mind, and death.

This formidable array is not intended to represent the objective phenomena in every case. In some, the disease will lack those extreme symptoms denoting collapse; in others, there will be no vomiting or purging; in others still, the cramp and great loss of heat will fail to be marked. These may be true cases of the prevailing epidemic; and if unaccompanied by the fully formed variety of the bad type, might leave us in doubt as to the pathological character of the ailment we were called to treat. Practically speaking, there are unfortunately so many fatal attacks answering to the first description, as to leave no diagnostic doubt in the physician's mind.

We may assume cholera to be the result of some specific poison, from the fact that it has been known to extend to other countries than the seat of its original production, the latter differing from the former in climate, soil, and geological formation, not less than in the moral and physical attributes of the people inhabiting them.

We may state that this poison enters the system, perhaps by the respiratory mucous membrane in some cases, certainly by that of the digestive tract in others, and produces morbid changes in the blood and ganglionic nervous apparatus. The materies morbi probably undergoes very great multiplication, as in the so-called zymotic diseases.

As yet, nothing demonstrable is known concerning the nature of the cholera poison. All that has hitherto been advanced in this direction is pure hypothesis. As such, it is not my purpose to consider it this evening. Whatever the essence may be, we know that it has a tendency to follow great routes of travel. It may be considered as proved that cholera can be propagated by human intercourse, and that under some circumstances it is the most important, if not the sole means of effecting its diffusion. This statement is not intended to assert the doctrine of personal contagion, against which theory the mass of authority seems to weigh.

It may be safely stated that where all proper hygienic and dietetic rules are observed, and where no localizing influence exists, cholera will not prevail in any great degree. When incubation is to be considered as an element of any choleraic problem, it is seldom that it exceeds three days. It may not exceed six hours. [William Budd.]

Whatever the essential cause may be, it has not been prevented in many instances from continuing its progress by natural or artificial barriers, such as quarantine, by the influence of season,

by adverse winds, by great elevation above the sea-level, or by any known atmospheric condition. Usually, its rate of progress is not regular. It is not, as a rule, more rapid than that of human travel.

In many instances, the theory of personal importation is insusceptible of proof. One attack furnishes no immunity from subsequent ones in the same person; nor is it probable that these are of less severity than the original.

Epidemics of cholera have shown a remarkable tendency to reappear in the same portions of the countries invaded. In the town of Leith, the first cases in the years 1832 and 1849 occurred under the same roof and within a few feet of the same spot. From this tendency, a valuable prophylactic hint may be drawn.

A remarkable coincidence has been observed in regard to places of election, between cholera and epidemic typhus and enteric fevers.

Cholera shows itself in the first cases of any general epidemic, at the outer commercial edge of the country invaded.

Its prevalence and mortality are inversely proportional to the altitude of the place affected. It rarely prevails unless at a temperature above 60° Fahrenheit. The Russian and Scandinavian epidemics furnish only apparent exceptions. The heat, filth, overcrowding of the huts, the want of nutritious food, and the drinking of tainted snow-water, furnish reasons why it should have remained during winter among the Muscovite peasants.

Impure water and emanations from fermenting animal refuse and excreta, are to be numbered as among the most certain and most powerful causes of cholera. From observations made by the late Dr. Snow, the mortality was three and a half times as great among persons drinking dirty water as among those who drank water which was pure.

Insufficiency of nutritious food; of wholesome air for respiration; intemperance in eating, and especially in drinking; over-fatigue of mind or body; the existence of previous disease; the abuse of purgative medicines; allowing the body to be exposed to chilling influence when overheated; prolonged exposure to great sun-heat; all depressing emotions—notably that of fear—constitute well recognised predisposing causes.

In cholera we have three forms sufficiently distinct to claim practical attention:

1. Those in which the premonitory diarrhœa lasts several hours, or even several days, and in which the loss of animal heat and impairment of circulation come on slowly.

2. Those in which, after a few characteristic discharges, the algid phenomena and collapse are manifested.

3. Those in which a morbid coldness and loss of pulse show themselves from the beginning. In such, vomiting and purging may be absent, although I have never failed to find the primæ viæ filled with fluid. They seem to commence in death.

These three varieties are fatal inversely in the order mentioned; the first being much the most frequent.

Loss of animal heat constitutes one of the most striking pathological changes in the disease. It is probably due to the anæmic condition of the lungs, which has been observed in patients dying before the function of calorification had been restored. In proportion to the severity of the algid phenomena is the disease fatal.

Death may ensue in a very short space of time. I have known it to occur in less than two hours in a previously healthy person. In India, Milroy saw soldiers—"hale, hearty men—seized, cramped, collapsed and dead, within five minutes."

At first, in any primary invasion of cholera, the mortality is apt to be very great. In the outbreak of some epidemics, nine-tenths of those attacked have died. This ratio tends to decrease until towards the close of the prevalence, when the great majority of cases end in recovery. Then it is that an endless list of infallible remedies is published.

The greatest number of sufferers and of deaths occurs in what are known as the *lower classes.*

The very old and the very young are less liable to survive the disease when attacked. Every cause tending to produce debility may be ranked among the unfavorable prognostic elements.

Finally, cholera obeys the general law of prevailing mildly in some visitations, and mortally in others, without our knowledge always enabling us to explain why the difference should exist.

From the concurrent testimony of the most able observers, we are amply justified in saying that we have it in our own power, if not to prevent the existence of cholera, at least to take away from it the character of a destroying pestilence. *This is to be done by prophylactic measures*, infinitely more important and more satisfactory than any known therapeutical resources within our power, when the fully formed disease has shown itself. Prophylæ is of two kinds: 1st, that which should be employed *before* the choleraic phenomena have been observed; and, 2d, that to which we should resort subsequently, in order to prevent the earlier stages of the malady, very often easily within our control, from passing into the algid stage, when the practice of medicine is so often nothing more than a melancholy contemplation of the phenomena of dissolution.

Attention should be given to the vessels and passengers arriving at Quarantine among which cholera prevails, in order that the sick may be properly separated from the localized tendency to production, as well as that appropriate purification of the transports, and of the persons, baggage, &c., may be insured. It would be a wise provision, until all doubt should have been removed, that no such possible sources of infection should be allowed access to what we know will be the localized centres of the epidemic.

The sanitary powers should make minute and systematic examination of every inhabited edifice and suspected district, in order to insure the observance of those hygienic laws relating to drainage, removal of garbage, overcrowding, and the like. Fortunately, our supply of drinking and cooking-water is so irreproachable in every way, that we need not insist on this subject so strongly as is necessary where pumps and wells are relied on.

In the event of cholera prevailing among us, the well tested experience of Great Britain and our own country shows that no one means is so important as the house-to-house system of visiting by honest and competent agents of the sanitary power.

We are thus enabled not only to put in practice the fundamental rule, "*Obsta Principiis*," but these agents have it in their power to disseminate such knowledge as the people among whom their labors would lie, usually fail to possess.

I purposely abstain from going more minutely into such directions as should be given to the public. They are so amply set forth in all the late treatises on the subject which now engages our attention, and may be so easily inferred from the general etiological principles hitherto enunciated, as to merit nothing more than mere allusion in this place.

I have purposely abstained from dwelling on the post-mortem appearances in Asiatic cholera. The anæmic lung; the abundant shedding of intestinal epithelium, giving its character to the rice-water discharges; the empty, contracted bladder; the dark, tarry blood, in cases rapidly killed by the disease, are familiar to my hearers. They are of interest in their way, but aid us not in our comprehension of the nature of the disease, nor do they furnish us assistance in our efforts to effect a cure by the administration of remedies.

The termination of an ordinary case of cholera may be by a subsidence of the gastro-intestinal symptoms; by a return of animal heat to the body; by a rapid filling up of the shrunken frame, and the restoration of the pulse, voice, and suspended secretions. Recovery may be prompt and uninterrupted, or it may be through a tedious sequela of adynamic febrile reaction, having great resemblance in its general character to typhoid fever. This condition may terminate in death by coma, by thoracic complication, by dysentery, or by exhaustion.

In the treatment of cholera, we must have reference to that of the premonitory or early stages, when painless diarrhœa is the most evident symptom.

To control this, rest in bed, warmth to the body, and some of the cordial astringents, of which opium forms a part, are usually sufficient, when conjoined with quiet, pure air, and proper diet.

To relieve the violent cramps so often encountered, ligatures of the extremities and anæsthetic inhalations may be essayed. I have had success to follow their use, and very many failures. The hypodermic use of morphia promises well. To allay the

vomiting, cracked ice, and cool, effervescent drinks, with epigastric warmth and counter-irritation, are to be resorted to. In some cases, rapid vesication and the application of morphia salts to the denuded skin have afforded relief.

It is well to allow cold water freely as a drink. Nearly all may be speedily returned by vomiting, but some small quantity may be retained and absorbed.

In collapse, use external warmth, and cold water internally. *Avoid large doses of opium.* The medicine, if retained, is apt to remain inactive, until with returning vitality absorption commences, when its cumulative action gives rise to poisonous effects.

In the reaction and secondary fever before mentioned, general principles of treatment are all that need be alluded to.

In convalescence, the patient's diet should be carefully regulated.

In conclusion, I would again apologize to the Academy for the meagre outline I have drawn this evening. The very affluence of material has rendered the task of condensation one of no little difficulty. I simply present this to my associates, with the hope that their research and experience may enable them to fill up the picture roundly and more completely than I have been able to do.

What we wish to be thoroughly instructed in is—How, most surely, shall we prevent the disease existing pestilentially? How, in the event of its prevalence, can it be most successfully managed?

Dr. John H. Griscom then read the following paper:

THE WHERE, THE WHEN, THE WHY, AND THE HOW, OF THE FIRST APPEARANCE AND GREATEST PREVALENCE OF CHOLERA IN CITIES.

Our country has just emerged from a most dreadful devastating war, in which hundreds of thousands of human lives have been sacrificed for the maintenance of sound political principles. The disorder which raged for four long years came upon most of us *unexpectedly*, and found us in a state of total unpreparedness; and only our native energies, and facilities of adaptation to new and unheralded emergencies, enabled us to save ourselves from destruction in a governmental point of view.

Happily, we have emerged from the terrible ordeal; and now in our convalescence, it is obligatory on us both as a people and as individuals to strengthen our constitutions by lives of purity and virtue, carefully watching events of every kind, and guarding against everything that may by any possibility endanger us.

We have therefore to ask ourselves the question—Is there any other enemy preparing for battle against us?—Is there any other

quarter whence we may be invaded by a desolating foe; and if so, are we prepared to combat him? To those important questions, certain events which have occurred during the past year, give a most emphatic reply in the affirmative, threatening the whole land with a devastation scarcely surpassed by that of our recent political intestine war. Already a subtle and malignant enemy has flaunted its flag in our sight, and for our present immunity from a repetition of the disastrous results of its four previous visitations, when 12,500 lives succumbed to its fury in this city alone, we may hold ourselves indebted, not to any foresight or precautionary measures of our own, but to an indulgent Providence which has given us another warning to amend our habits and modes of living, with the almost positive assurance if we do not, that the same penalty must be paid as before.

The purposes of the present allusion to this grave topic are, 1st, to expose the mode of approach and the manner of warfare of this subtle enemy of the human race; 2d, to show how readily it may be held in check and divested of its fatal weapons; *thirdly*, to correct some popular errors respecting its times and modes of attack; and *lastly*, to demonstrate the importance of keeping on constant duty a vigilant and intelligent picket-guard and police, not only to give notice of its approach, but to maintain an incessant observation of our own camp and barracks, that the occupants may be ever ready to meet the wily foe.

The magic letters C. S. A., recently so proudly used in the Rebellious sections of our land by those who would have subverted the best government on earth, are the initials of that other arch-enemy—*Cholera Sinister Asiatica.*

Though of foreign birth, and bearing a foreign name, it is nevertheless, like a large majority of its cis-Atlantic victims, an adopted child; in respect to which that often repeated Irish bull has become a truism; it has been here long and often enough to be a native.

Strictly speaking there are, as we believe, two special influences at work in the causation and extension of this disease. The first of these is a general atmospheric condition, the precise character or origin of which has not been determined. The other influence is of exclusively local origin, from conditions clearly defined and understood.

To illustrate this, we may employ the figure of the "Shears of Fate," of which one blade represents the terrene, the other the atmospheric circumstances. In the absence of one or the other of these parts, the remaining one is powerless, or at least its efficiency is so greatly diminished as to be of very little consequence. Thus though the terrene exhalations be present and even abundant, if the choleraic atmosphere be absent, we have no cholera; and *vice versa*, should the choleraic atmosphere visit a place entirely free from any foul terrene exhalations or other local causes, the same negative result ensues. One blade alone of the shears,

so far as the cholera is concerned, is a harmless instrument. But by a combination of these circumstances, by a union of the meteoric epidemic influence with the emanations from undrained, overcrowded, filthy tenements, unswept streets, damp cellars, noxious manufactories, and the thousand other nuisances of neglected cities, we have the two blades of these destructive shears in fatal conjunction, ready to divide the vital chords in thousands of cases. It must be understood, however, that this figure is applicable only to the disease under discussion. No fact is better established in the science of medicine than that typhoid and typhus fevers, and many other fatal disorders, are the direct results of these same local circumstances. To demonstrate their extent and efficacy in poisoning the people, it needs only to be stated that of the 25,645 deaths which occurred in this city in 1864, more than one-half were caused by diseases either directly produced, or greatly aggravated, by the shockingly neglected and foul conditions alluded to. This blade of the fatal shears is therefore constantly on hand, ready at any moment to combine with its colleague of the air, and by their union to sever the vital chords of a larger number of denizens than ever before; for never was our city more ripe than now for the harvest of death by cholera. The localities of cholera are peculiar, its favoring circumstances well understood, its victims almost exclusively of a certain class, and *these are all identical with those which are known to be especially prone to and provocative of typhus fever.*

The applicability of this figure of the Shears of Fate, finds a striking confirmation on board of emigrant ships. It cannot well be controverted that in the constitution of the general atmosphere which covers the ocean, there is sometimes something of an occult or mysterious character which will produce this strange disease; yet the opinion cannot be dismissed or disregarded, that there must be in the *local* atmosphere of the crowded emigrant ships some powerful predisposing and supporting causes, otherwise it would not prevail so uniformly among people of a particular class and character. On one side of the narrow line which divides the cabin from the steerage, we find total exemption from, and on the other side great proneness to the disease. It enters the ship by the hatchways, and not down the cabin stairs. The reason for this marked difference between these two parts of the comparatively diminutive *cell*, denominated a ship, is too well known to require an explanation here; it will suffice to allude to the notorious fact, that the steerage is the germinating soil, and the hotbed for the development and maturity of that most serious form of typhus, *ship-fever*. It would seem as if this last-named distemper, self-generated though it be by the victims themselves, was a too mild penalty, and that Providence had deemed it necessary to send in these latter days a swifter-winged messenger of death, to punish His creatures for their continued violation of his laws, given for the preservation of their health and lives; a

principle applicable not to ships alone, but to cities also. I shall now quote a few examples in illustration of this theory, from the history of cholera as it appeared in some of the largest, wealthiest, and best located cities of Europe and America, and draw such inferences therefrom as are fully justified, and will serve as lessons for ourselves at home.

First, however, allow me to say that the prevalent idea of cold and frosty weather being fatal to the cholera poison is not wholly correct, inasmuch as several instances have occurred of its commencement and continued prevalence in midwinter.

In proof of this I cite the case of the village of Carnbroe, in Scotland. Its population did not exceed 1,200, chiefly engaged in the manufacture of iron, who were all in good health up to the night of the 31st of December, 1848, when cholera made its appearance, and caused more sudden and severe suffering than in almost any other locality known to the official reporter, Dr. Sutherland. It continued to rage until the end of February, in which time 1,100 out of the 1,200 were attacked. "This severe outburst of the disease took place after the people had been drinking for several days."

In his admirable and minute report on the cholera of 1848 and 1849, as it prevailed in Great Britain, Dr. Sutherland says, in speaking of Edinburgh and Leith: "True to the laws by which epidemics are governed, *it followed the usual track of fevers* by which those towns are scourged, locating itself in the same filthy closes, occupying the same ill-ventilated, overcrowded tenements, not unfrequently carrying off its victims from the *self-same rooms* which its fatal ravages nearly depopulated in 1832." He adds: "The most important practical point for us to know is, that the severe manifestation of the presence of cholera described above does not take place over the whole district covered by the epidemic influence. Were this the case, a large proportion of the people in affected countries must necessarily perish. All experience has, however, proved that a certain portion escape, while another portion are destroyed, and the fatal outbreaks of the disease are invariably connected with one or more of the following local defects: overcrowding, dampness, filth, want of ventilation and atmospheric pollution, proximity to graveyards and other nuisances, pigstyes, offensive sewers, etc., narrow, closely built, and confined neighborhoods, bad water, natural defects of situation; the impregnation of the subsoil of towns with organic matters from filthy streets, cesspools, etc.; imperfect sanitary works, etc.

"The city of Edinburgh was the first part of the United Kingdom attacked by cholera in 1849. The sanitary condition of the affected districts was at the time most defective, even in regard to the removable causes of disease; but it is to the credit of the local authorities that they proceeded immediately and vigorously with the abatement and removal of nuisances, and with the cleansing of filthy streets and closes." I need not here enumerate

the different methods by which the purification of that city was effected; suffice it to say, that the lesson derived from the cholera visitation of 1832 was not lost on this occasion, and that, in consequence thereof, "the deaths by cholera in this epidemic were only about one-half of those reported during the epidemic of 1832; while in all those towns where equally effective measures were not adopted, the mortality was very much greater from this than the former epidemic." Sad is it to be unable to include New York in the same category as Edinburgh; but truth demands its inclusion with the other class; its cholera mortality in 1832 having been 3,513, and in 1849, 5,071. The wisdom of the Scot was lost upon the Yankee.

A most instructive example occurred in *Taunton*, which was visited by cholera in the beginning of November, 1849. Its population did not much exceed 16,000. At one extremity of the town is situated the workhouse, and at the other the county prison; the sanitary conditions of which differed most materially from each other. The situation of the workhouse was badly drained, the refuse being carried by a sewer to a cesspool in the garden, which was uncovered until just before the cholera began. Its internal arrangements were equally defective, there being no ventilation, the ceilings only eight feet nine inches high; the water-closets opened into the wards or staircases; and the means and appliances for the personal cleanliness of the inmates were equally defective. Great mortality had prevailed there for many years. There were 276 inmates when the cholera broke out on the 3d of November, the first case passing into hopeless collapse in ten minutes. Up to 4 P.M. on the 5th (two days) no fewer than forty-two cases and nineteen deaths had taken place, and in one short week sixty of the inmates were swept away.

The county jail, at the other end of the town, presented a most striking contrast in every respect. The prisoners' cells were large, each having 819 cubic feet, with a good system of ventilation besides; the means of personal cleanliness were abundant and their use strictly enforced, in consequence of which *not a solitary case either of cholera or diarrhœa occurred among the prisoners.* An equally interesting and curious fact occurred in the children's schools. The girls' schoolroom was a slated shed fifty feet long, nine feet ten inches broad, and seven feet nine inches high to the top of the walls, over which was a sloping roof. In this miserable place were huddled together 67 children, with only 68 cubic feet of air to each. The infant school, under the same roof, was only fit for a coal-cellar. Of the 67 girls in these rooms, 3 were removed when the disease appeared; and of the remaining 64, 33 were seized with it, of whom 25 died and 8 only recovered. The other 31 only escaped by being removed, part to a large airy schoolroom in the neighborhood, and part by parents and friends to different places. The boys' apartment was rather worse than the girls'. In the girls' school the windows were never broken;

but the boys, who were good and obedient in other respects, could not be kept from breaking the windows—the result of which was only 10 deaths among the boys, while 25 of the girls were lost.

The people of the town, 16,000 in all, situated between the two public institutions, escaped with some cases of diarrhœa, but without a solitary instance of cholera; the whole town thus affording, in its various parts, a most powerful instance of the value of preventive sanitary science.

An hour might be occupied in quoting similar exemplifications of the favorite localities and habitudes of cholera from foreign sources; but as its *domestic movements* and the character of its visitations in our own land are more interesting to us, and can be perhaps better understood, the investigation will be transferred to this side of the ocean.

The knowledge of the place and manner of its first cis-Atlantic visitation is sufficiently familiar to you all to render a minute account unnecessary. It first struck the American shore in 1832 in Canada, and thence moved rapidly southward. Reaching this city in a few weeks, it made its first appearance, not among that class nor in the localities inhabited by those persons by whom it is supposed by some theorists *to have been brought here*, viz. the travelling community—but the beginning of its devastations was among those who scarcely ever leave their homes, wretched and filthy in the extreme though they are. Nor did it appear first upon the borders of the city, by the wharves and docks, the places of direct commercial intercourse, where, if contagion were the only or the principal means of its transportation from place to place, it should have first presented itself. But its *début* was in the very heart of the city, and among the class who are especially prone to typhus fever; and its progress then was very erratic, appearing in similar localities in different and distant parts, demonstrating the universality of the atmospheric influence, which combined with the local causes from day to day where these were most rife and abundant, until at last it became almost universal. In 1832 it first appeared in the Fourth Ward, in Cherry and Roosevelt streets, but almost simultaneously in the Third Ward, in Reade, Washington, and Duane streets, and in the Sixth Ward at the Five Points; which, together with a few other places, similar in their filth of houses and streets, were the centres from which the disease radiated, its intensity and frequency varying with the distance from these points.

Nashville, 1849.—Dr. Buchanan.—The first case occurred January 20 in a house very near the bank of the river, which had just risen so as to inundate the upper and lower parts of the town, and in its partial subsidence had left this house, with others, almost surrounded by water. Its occupants were of the lowest and most indigent class, and the first case was a man of intemperate habits, who died in a few hours. On the next day another in-

mate of the same house, and of similar habits, was seized, and though moved to comfortable quarters, he succumbed. The third case was in the same house, an associate of the former two; and though removed with all the families who occupied the house to better quarters, in a more elevated part of the city, he collapsed and died in thirty-six hours. The black population and the lower class of society—the most ignorant and indigent—were the greatest sufferers.

From its commencement to its termination, there were certain localities in which the disease raged with peculiar violence. These points seemed to act as foci whence the disease radiated to other parts. The first of these which attracted public attention was a house, Seventh and St. Charles streets, occupied by several Irish families, some inhabiting the damp basement. Early in January, a case of cholera, originating on the river, was taken to this house and died. Soon after it broke out among the other inmates, six or seven of whom died in ten days or a fortnight, when the house was abandoned. The character and habits of these persons were such as to render them fit subjects for cholera or any other disease. Another ill-fated location was in a row of small two-story frame buildings, north-west side of Green street, between Sixth and Seventh streets, densely inhabited mostly by Irish. Here the disease prevailed violently; scarce a family escaped without one or more deaths, and some were almost entirely swept off. The only peculiarity about the situation of these houses is, that they are built near the ground, and with lots so exceedingly shallow as to bring the outhouses within a few feet of the backdoors. Still later in the season, the disease prevailed with fearful violence in a row of two-story frame and brick houses, in St. Charles street, between Eighth and Ninth streets, numerously occupied by mechanics and laborers, whose condition was somewhat better than those before mentioned. Nearly the entire population of this block was swept off, 192 deaths occurring in the row. But the street opposite had never been paved; *the situation is damp, the cellars were filled with water, and the premises otherwise filthy.*

In another neighborhood, Biddle, O'Fallon, and Eighth streets, and Biddle and Tenth, the disease raged with unmitigated severity, sweeping off hundreds. The population, almost entirely German and Irish, were herded together in large numbers. Near by also are large ponds of stagnant water, some of which cover twenty or thirty acres of ground.

But by far the most fatal locality was that known as *Shephard's Graveyard* (so called from the number of deaths which occurred there), being in the south-western part of the city, and embracing three squares, *the former bed of Choteau's pond.* The situation of this place is low, damp, and filthy, and teemed with a population of the poorest and most destitute character. Here, as might naturally be expected, the cholera raged with unmitigated violence, and carried off its scores and hundreds. Dr. Alleyne, who had

charge of this district during the epidemic, stated that many cases occurred without the slightest premonitory diarrhœa, and terminated fatally in an unusually short time. There were several other localities in different parts of the city in which the disease was more fatal than usual, and as a general fact it may be stated that it prevailed most in those parts in which the largest number of persons were herded together, where the streets were unpaved, and where there was the greatest amount of filth and moisture.

"It is perhaps not too much to say that at least *seven-tenths* of the entire mortality occurred among the Germans and Irish." The same result may be anticipated should the cholera revisit New York.

How ill prepared St. Louis was for this dreadful visitation may be judged from the following statement of its condition by the authority above quoted:

"Notwithstanding the warning given by its gradual approach, and the length of time thus afforded for placing the city in order for its reception by a thorough cleansing, and by removing every source of disease, as well as by establishing and maintaining a vigorous health police, and preparing suitable hospitals for the reception of the indigent sick, there was manifested a singular and most reckless apathy on the part of our authorities. The city was never in a more filthy condition, and yet no adequate steps were taken towards cleaning it, until at length public indignation was aroused to such a pitch by the cruel inaction of the authorities, that mass meetings were assembled, and the people, in their sovereign capacity, demanded of them, in language not to be mistaken, *either to do their duty, or at once to resign.*"

Louisville, 1849, Dr. Bell.—The first case occurred May 1st, in a house located in a very unhealthy position below the summit of the second bank of the river, and in the immediate neighborhood of yards that received the filthy washings of the more elevated ground above. In 1832 and 1833 the first cases appeared in the identical square in which the case above alluded to occurred in 1849.

Association of the Disease and Filth.—In the beginning of June intermittent fever, dysentery, remittent fever, and cholera began to show themselves in low, filthy, and undrained localities. In one house a family of eight persons, living in the most wretched filth on the banks of Beargrass Creek, and all occupying the ground floor as lodging-rooms, were attacked with cholera, and five of them died; the others were saved by being removed from the locality. A family, however, living next door, occupying the second story of the house as lodging apartments, escaped without any sickness. Another family in the same locality removed to one no better. The house to which they removed was a new one, with two rooms, twelve by fourteen feet in size, and the plasterer had just finished his work on the rooms, and nine persons undertook to lodge in these two small rooms, with the atmosphere

reeking with moisture from undried plastering. The removal took place on Saturday; on Sunday the wife of one of the parties was taken sick and died with cholera. Her corpse was placed in one room, and the eight survivors undertook to sleep in the other. By Thursday but one of the nine was left alive, and his life was despaired of, but being removed to a hospital, he recovered slowly. Sporadic cases of this kind continued to occur, and in the neighborhood of piles of decaying hemp offal the cholera was especially malignant.

About the 24th of July, the pestilence broke out with great violence in the lower end of the city, the site of an old pond, in Market, between Tenth and Eleventh streets. Another pond, north-west of the square, was still in existence, and a rope-walk was on the eastern edge of this pond. An immense mass of refuse hemp had been thrown into the edge of the pond, and the evaporation of the water had exposed a mass of this decaying vegetation to the action of the sun. A good deal of vegetable filth in a moist condition was on the east, or opposite side of the rope-walk. The cellars between Tenth and Eleventh streets were generally full of water, and the ground was so saturated with water from the heavy rains of the spring and summer, that upon pushing the end of a walking-stick down a few inches water would come bubbling up. Pools of water stood under old frame houses that had no cellars, and all walls showed the marks of extreme dampness. *The sewer on Tenth street was choked so that it could not carry off the water.*

An interesting and important fact pertaining to this visitation was the *sudden invasion of the disease, but its causes not operative in the daytime.*

During the night of the 23d of July (Monday), cholera commenced its ravages in this small district, and by Tuesday night at eleven o'clock there had been fifty cases and thirty deaths. The remainder died after eleven o'clock and during the next morning. From eleven o'clock on Wednesday up to nightfall there was not a new case, but immediately after sunset there were fifteen new cases. On Wednesday and Thursday nearly all the survivors were removed out of the district into healthy quarters, and by the free use of sand and lime the infected square was purified. A large number of citizens labored diligently in this work, who were assured that there was no danger in the daytime, but that they must not expose themselves to the night air. Of the large number that worked in the daytime, from Tuesday to Saturday afternoon, not one was attacked with the disease; but a number of those who exposed themselves at night died, thus giving conclusive proof of the malarial origin of cholera. A few of the survivors, who were scattered over various parts of the town, died at various periods for more than a week, but no one of the families among whom they died suffered any attack of cholera. We neglected to say in its proper place that on the night the cholera

broke out the west wind was blowing over the decaying hemp on the edge of the pond, carrying the malaria directly upon the south side of the infected square, which side suffered much the greatest mortality.

It is worthy of note that the measures taken to destroy malaria checked the cholera, so that it never extended beyond its first outbreak, and the epidemic was brought to a close on the second day!

"Two weeks after this, another outbreak took place towards the upper end of Jefferson street, where an old pond stands in the middle of the square, with its edges covered with vegetable filth. The marshy places were in a horrible condition. On the northwest corner of the square is a number of miserable shanties, and behind them, extending the whole width of the lots on which they stand, was a green pool containing masses of vegetable filth. Under the floors of these shanties the water stood six feet deep, and nearly all the cellars in the square had from three and a half to five feet of water in them. The south wind blowing over this marsh, scattered the malaria to a considerable distance over several squares, producing cholera. A resort to the same measures that had been successful in the lower end of the town was made, and they succeeded in the same prompt and efficient manner." Says Dr. Bell, the reporter of these facts: "The malarial origin of cholera has been as clearly demonstrated in this city this summer as if it were written by sunbeams."

Those places in Louisville which bore the brunt of the cholera in 1833, and which have been improved so as to be dry, clean, and airy, did not have a case in them in 1849 or 1850, except a single case in one of these improved squares. But those places which were scourged in 1833, and which remained in the same state as then, were scourged again in 1850.

Sandusky, 1849.—*Influence of Fright.*—The presence of the disease continued as an epidemic one month from July 8th, committing in that short time a most fearful mortality. Out of a population numbering July 3d 5667, there were three hundred and seven deaths, two hundred and eighty-five of which were from cholera, being nearly six per cent. of the whole; and it is estimated that when the disease had reached its height, the number of persons in the town did not exceed one thousand. This excessive amount of sickness and mortality was doubtless in great part due to the terror which the presence of the disease struck into the hearts of the people. So great was the panic that business was suspended, the post-office was closed, and the people fled.

Buffalo, 1849—Dr. Flint.—*Population most susceptible.*—The parts of this city where cases were most numerous are confessedly less salubrious than those in which the disease prevailed in a less degree, and, at the same time, contained a larger proportion of the classes of population furnishing the larger number

of victims. The sections in which the epidemic prevailed most, and with greatest virulence, were: 1st. Streets in close proximity to the canal, densely inhabited, and abounding most in poverty and vice; 2d. A part of the city known as the Hydraulics, in which miasmatic affections are somewhat rife, and the population, generally speaking, not of a character to be peculiarly exempt from epidemic diseases; and 3d. In a part lying in a north-eastern direction from the centre of the city, in which the population consists almost wholly of German laborers.

Newark, N. Jersey, Dr. Clark, 1849.—Although the remote causes are enveloped in impenetrable mystery, among what we do know are these: That its favorite place of development is where *filth abounds, where many are crowded together in too small a space, and where noxious exhalations arise.* In 1832 many cases occurred in the *very same localities* that were visited this year. In two points in the East Ward, *Gallagher's Court* and *Durand street*, both remarkable for their crowded and filthy condition, the cases were numerous and fatal. The most fatal localities in West Ward were *Maiden Lane* and *Washington street*, and the vicinity of the dead stream that flows through that part of the city, and the low grounds in the rear of the "*Nine Row*" in Summit street. Other localities, where streams flow on the surface, and filthy and badly ventilated houses exist, are mentioned as those to which the cholera most inclined.

Philadelphia, 1849.—The history of this city presents one of the most praiseworthy efforts and successful results of prudential anticipatory sanitary measures of any on record. Previous to the outbreak of cholera in the United States, and while it appeared to be progressing slowly but surely over Europe, the Board of Health, anticipating for Philadelphia an attack similar to that which it had experienced in 1832, endeavored to guard, if possible, against its approach by instituting timely measures for its prevention. With this object in view, the Sanitary Committee of the Board made a report in November, 1848, recommending the rigid enforcement of sanitary appliances to check its progress, if possible, or at least shorten its duration and mitigate its severity.

The result of this action was, from October, 1848, to October, 1849, a total of 6573 nuisances abated. The effect of this wise precaution and energetic movement, so notably in contrast with the operations of most other places, was unquestionably manifested by a marked degree of moderation in the prevalence of cholera, both in manner and extent. The two first cases were announced on the 30th of May, on a canal-boat which arrived the day before at the upper verge of the city, in the Richmond district. The vessel upon which these cases occurred was in so exceedingly filthy a condition, that she was sunk. The third case occurred on the same day on the lower verge of the city, the district of Southwark, a distance of three and a quarter miles from

the former ones. The man had been in New York a few days previously, where cholera was then prevailing. On the next day a case occurred in the city proper, a quarter of a mile from the third case. The next case was on the following day (June 1st), three-quarters of a mile from the preceding one, and the sixth case on the same day, near the Schuylkill, at least one and a half miles from any of the others.

Thus in the first 17 days 23 cases occurred, of which none, with the exception of the two first, were within a square of each other, although every district but the Northern Liberties and Penn had at least one case. Its prevalence independent of contagion was thus clearly manifest.

The disease ceased as an epidemic on the 18th of August, though sporadic cases continued to occur till September 22d, by which time 1,022 deaths had occurred, which would doubtless have been doubled but for the sanitary precautions and care taken.

The population of Philadelphia was at that time 450,000, giving a proportion of a small fraction over 1 to every 450 inhabitants. In this same year the deaths by cholera in New York city were 5071, in a population of 500,000, or one to every 100, being 450 per cent. greater.

The fact may be here stated that already the Health authorities of Philadelphia have again led the van in preparing for the threatened invasion of this disease, although the necessity for so doing there is much less urgent than here and in many other places.

One marked distinction, which renders Philadelphia less susceptible in general than New York to invasions of cholera and other epidemics, is the less crowded and more wide-spread dispersion of its population. A short time since, the census statistics showed that with a population 100,000 less, that city contained 30,000 houses more than this.

Philadelphia has for many years been blessed with a Board of Health consisting of eleven persons, a majority of whom are men selected by the appointing powers with sole reference to their long familiarity with sanitary science, and its practical application in both public and private localities, and who maintain a constant supervision over the whole city at all times, with reference to the prevention of all kinds of preventable diseases. The selection of a member of the Board of Health on political grounds alone, would be there regarded as absurd and ridiculous as to choose a family physician on the same principle, without reference to his professional ability.

But the most striking of all the instances on record in this country in proof not only of the value of preventive sanitary measures, and of the effect of their neglect, but also of the truthfulness of the figure with which this paper was commenced, viz. the Shears of Fate, was presented by the city of Baltimore:

"During the spring of 1849, serious apprehensions were felt lest the cholera should visit Baltimore, and these apprehensions were ripened into certainty when it reappeared in New York in May, and in Philadelphia a short period later.

"The public authorities, as well as the citizens generally, exerted themselves with commendable zeal to ward off the severity of the anticipated attack, by placing the city in as cleanly a condition as possible. Additional sums were appropriated by the municipal authorities to enable the health department to perform its duty; and the police were constantly urged to diligence in seeking out and causing to be removed all sources of filth or nuisance.

"A communication from the Board of Health was laid before the mayor, who deemed the matter of sufficient importance to convene a special session of the city councils, and in consequence, says Dr. Wynne, the impression was very general that uncleanliness had much to do with the spread of the disease; and under this belief I think I may safely assert that never before or since was the city so thoroughly purified, or so well prepared to receive its unwelcome visitor as during the summer months of the year 1849.

"About the 12th of June, 1849, and while cholera was prevailing to a considerable extent at New York, Cincinnati, and other places north and west of Baltimore, diarrhœa, and affections of a kindred character, became very prevalent, but yielded easily to medical treatment. I remarked at this time another peculiarity, which I experienced in my own person, and which was of universal occurrence; it was an indefinable sense of oppression, not amounting to pain, over the whole region of the abdomen, in all those who had not a positive attack of diarrhœa, reminding the person constantly of the presence of such a part of the body. In about two weeks from the beginning of this prevalence of diarrhœa, all unusual symptoms of it began to subside, and cholera appeared at Richmond and other towns south of Baltimore.

"I felt assured at the time of the prevalence of this diarrhœa, that the poison which produced cholera pervaded the atmosphere; that it brooded over us, and that we were affected by its presence; and I consequently anticipated, momentarily, an outbreak of the epidemic. These symptoms subsided, and I felt that the fuel necessary to coöperate with this poison did not exist in our city, and that it had passed over and left us unharmed.

"I consequently advised a large number of my friends, who were about to leave town under the apprehension that cholera would soon appear, not to do so, assuring them in the most positive manner of my belief that we should escape the disease, for the reasons already given. They consequently, for the most part, remained, and partook of the ordinary diet usual at this season of the year, including ripe fruits, from which no injurious consequences resulted. On the contrary, the most severe attacks of dysentery I was called upon to attend at a later period in the

autumn were among those who had observed a rigid system of diet from apprehension of the appearance of cholera.

"On the 1st day of July, an old man named John Cranmer, an inmate of the Baltimore almshouse, suffering from an ulcer, was attacked with unequivocal symptoms of cholera, but recovered.

The almshouse is situated upon a very beautiful and healthy slope, about two miles north-west of the populous portion of the city, and is in immediate contiguity with the country-seats of several of the wealthy families of the town. It is surrounded by a farm of upwards of 200 acres belonging to the establishment, for the most part devoted to cultivation, and from its position is eminently salubrious.

"The main building was originally the country-seat of a wealthy citizen, who erected a costly mansion upon the spot on account of its beauty and healthfulness.

"The whole present principal frontage is to the south, of about 800 feet, of which either wing occupies upwards of 300 feet, and is about 30 feet wide. The main building is occupied by the attendants of the establishment, the east wing by the male, and the west wing by the female inmates.

"Previous to the appearance of this case of cholera, the visiting physician, Dr. Thomas Buckler, was quite earnest and emphatic in his directions to have the whole establishment thoroughly cleansed, so as to remove all exciting causes of the disease from within. His directions had been carefully complied with, so far as ventilation, removal of nuisances, and cleanliness, were concerned.

"On the morning of the 7th, a man named Peter Grow Ruburg, who had likewise been an inmate of the house for some time, was attacked by cholera and died.

"On the 11th, the disease appeared on the women's side of the house; the female attacked died.

"On the 12th, two new cases occurred, one on the men's side, and the other on the women's; both proved fatal.

"On the 13th, a man was attacked and died; and on the 14th, ten cases appeared in the men's wards, and three in the women's.

"In regard to all these cases, I may remark in one word, that they were all old inhabitants of the establishment, and had unequivocally contracted the disease on the spot.

"Dr. Buckler, who was sadly puzzled at not finding any local cause to warrant the spread of the disease within the inclosure, on the morning of the 19th determined to investigate the subject fully, and made for the first time a survey of the premises outside of the wall, surrounding the building upon its northern exposure. He commenced his scrutiny on the east wall, and discovered the cesspool connected with the black people's hospital overflowed, and in a very filthy condition; continuing the inquiry, he found that the drainings from the extensive pigsty had covered a large space with an offensive and putrescent deposit, which covered a

large surface of rank grass and rotten weeds, and then found their way to a filthy pool, the contents of which seemed to be in a state of actual fermentation. The next object which he met, was a large surface covered with the overflowing contents of the men's privy, and a short distance from this a similar one of greater depth, containing the washings from the dead-house; and still further on, a much larger one, at least 30 feet wide by 40 feet long, and 3 feet deep, about 20 feet from the north wall, leading from the wash-house, and communicating with a similar one charged with the contents of the cesspool attached to the insane female ward. In short, the whole space included between the ravine and the wall, upon its north side, was one putrid and pestilential mass, capable of generating, under the ardent rays of a midsummer sun, the most poisonous and deadly exhalations.

"It will be remembered that in the building running from north to south, on the females' side of the establishment, the lower story had a communication by means of a door to the north, and quite near to the cesspool, and that the remaining stories presented to this point of the compass a blank wall. The whole building was occupied by its inmates as before described. In the lower story were seventeen lunatics, *all of whom were attacked with cholera, and all died. The remaining inmates of the building entirely escaped;* not a single case of the disease appeared in either of the other three stories.

"The female wing of the main building is partially protected from the north wind by three intervening rows of trees; and although the number of occupants was greater than on the men's side, yet the number of attacks was considerably less.

"While the cholera prevailed at the almshouse, a constant intercourse was kept up between the inmates of the institution and their associates in town. Many left to reside in the city, and in one or two instances persons so leaving were attacked by the disease, and were brought in this state back to the almshouse. The foundlings were likewise removed from the almshouse, and kindly treated by some humane gentlemen in town in their own houses. The houses of Mr. Vansant, the president of the institution, and of Mr. Grafton, one of the trustees, were generously thrown open for this purpose. One of these children only was attacked by the disease; yet notwithstanding this constant intercourse, the disease did not spread, but confined itself to its favorite haunt.

"No cases of cholera were reported by the Board of Health, yet four cases occurred, presenting such unequivocal symptoms of the disease, as to leave but little doubt as to its true nature. Two of these occurred in a wretched tenement, in a very filthy condition, at the corner of Second and Gay streets, near the docks. The patients, a mother and son, both died; they were Germans. The next case was that of a man who lived directly opposite the City Hall. The window of his sleeping apartment looked into an

alley which was in the most filthy condition. The fourth case occurred at Barnum's Hotel, the largest and most fashionable one in the city, in the person of a traveller; he died in 12 hours after his attack. No other cases occurred; and upon the subsidence of the disease at the almshouse, no new cases occurred.

"The facts presented at the almshouse certainly implicated the ravine loaded with filth in its spread at that establishment; and the immunity enjoyed by the populous city in its vicinity, notwithstanding the usual wretched alleys and miserable inhabitants found in all large towns, clearly demonstrated the efficiency of sanitary means in warding off the attack. It is true that the conditions of the atmosphere along the whole seaboard were not favorable to the spread of the disease, but yet it did occur, and proved fatal to a large number of the inmates of the almshouse. No one with these facts before him can doubt that Baltimore would have been visited by cholera as an epidemic, but for the wise precautions taken by the public authorities, seconded by the efforts of the citizens."

Have we not, in these instances, incontrovertible demonstrations of the inestimable value of sanitary appliances, and do they not show the splendor of the opportunity offered to the administrators of our Metropolitan Board of Health for the achievement of similar glorious results, the demand for which was initiated in this Academy twelve years ago; the bread which it then cast upon the waters being now returned to it?

I have alluded to the fact that the circumstances and localities which develop typhus fever in ordinary seasons, are identical with those which give rise to cholera in the years when its peculiar atmospheric influences prevail. The causes of typhus are purely local, and constitute but one blade of the Shears of Fate; while the second blade forms a conjunction with the first at particular seasons only, producing the more severe and rapid result denominated *cholera*.

The question of the contagiousness of cholera, and the value of Quarantine as a means of preventing its access to any city or country, has long occupied the profound attention of the most attentive observers and best thinkers, both in and out of the medical profession; and until its recent reappearance in Asiatic and European cities, the negative opinion as to its contagiousness seemed to predominate.

This question has been recently revived with much vigor in European localities, especially in Constantinople, and its discussion renewed on this side of the Atlantic, through a communication from the United States Legation in Turkey, addressed to our Secretary of State at Washington, urging the authorities of this country to guard against the cholera by "the most rigid Quarantine regulations." This communication inclosed and endorsed a newspaper article, in which the doctrine is stated and enforced, that cholera is contagious, and can be prevented by Quarantine

regulations, though it acknowledges that the Quarantines of the East (well known to be of the most rigid character) have failed utterly to prevent the spread of the disease.

In connexion with this branch of the subject, one of the ablest and most experienced practical sanitarians of the United States, Dr. E. M. Snow, of Rhode Island, uses the following language:—

"The cholera cannot prevail as an epidemic without the presence of a certain mysterious condition of the atmosphere. Of this atmospheric condition little is known; but it is known that its presence or absence is not affected or regulated in the slightest possible degree by quarantine measures. Vessels may have the cholera on board, and may land the sick on our shores; but the disease will never become epidemic, nor spread among the people until that mysterious atmospheric condition is present. But that atmospheric cause may come and may be present, and the disease may spread epidemically, without any connexion with the arrival of vessels from ports where the disease exists.

"If not a single vessel should arrive in our ports during the next five years, it would not make a single day's difference in the presence or absence of the cholera as an epidemic.

"The city of Providence has had some peculiar and instructive experience in relation to quarantine measures for the prevention of cholera. On the appearance of the disease in this country, in 1832, a most rigid quarantine was established in Providence, not only for vessels from foreign ports, but also for steamboats and vessels from New York, and even for passengers by stage coaches. No person was permitted to come into the city within ten days after leaving any place where cholera existed. The quarantine was so rigidly enforced as to stop all direct communication with New York, and to excite the indignation of the country generally. Nothing of the kind would be possible, if desired, at the present day. But, at the same time, a most thorough sanitary examination and purification of the city was made. What was the result? The cholera appeared here, and the first victims were four persons, in one house on Eddy's Point, who all died within two days. A most thorough investigation was made in relation to these cases, and the fact was established, that those persons had no communication, directly or indirectly, with any person or thing, from any place where cholera existed. So much for the rigid quarantine."

But the internal sanitary measures were so effectual that the cholera did not become epidemic here that season, and but a few cases occurred; and except in the first house where it appeared, there were not two cases in any one locality in this city.

Having presented thus my own views of the circumstances under which this comparatively modern foe of human life derives its extraordinary power—which might be further illustrated by scores of other cases similar to those already quoted—I will conclude this branch of the subject by another extract from the pen

of the distinguished sanitarian last quoted, confirmatory of the applicability of the figure of the *Shears of Fate:*

"The causes of cholera are, *first*, the mysterious atmospheric condition of which we have spoken; and *second*, local conditions of filth and impure air. The disease cannot prevail without the combined presence of these two causes. The first cause is beyond human control; it advances and recedes in obedience to laws of which we know nothing: it crosses continents and oceans as freely as the winds of heaven, and it would be as reasonable to attempt to control the movements of the east wind, as to attempt to control the movements of this cause of cholera, by quarantine restrictions. But the second cause of cholera, and without which the first would be comparatively powerless for evil, is, to a very great extent, within our control, and here is where our duty lies."

In relation to the treatment of this disease, having occupied so much time already with the discussion of its causes and habitudes, I will conclude by quoting an interesting, and what appears a very valuable essay, read before *l'Academie de Médécine* of Paris, by Dr. Worms, Physician-in chief of the Military Hospital of Gros-Caillou, which I have rendered from a French journal:

"The study of the numerous cholera epidemics which have visited Europe within the past thirty-five years, has not been sterile; it has furnished science with matter of incontestable importance.

"One of its most precious gifts is the establishment upon a positive basis, of its prophylaxis and its relation to public hygiene in localizing the toxic element, its transmission by the matter of morbid dejections, and in signalizing as the most decided auxiliary of this poison, the emanations of animal and vegetable substances in a state of putrefaction, the gases from privies and from stagnant waters.

"Another ascertained fact, and not less important, is that sudden and severe attacks of cholera, without premonitory indications, are a very rare exception; and that in a great majority of cases, a characteristic trouble of the digestive functions, and simultaneously of those of innervation and circulation, always precedes for some time and announces the invasion of the grave form of cholera.

"It is impossible not to admit that these prodromic troubles (justly entitled choleriue) are the effect of the slow and gradual action of the toxic agent, which has not yet found, either in the local circumstances or in the individual predisposition, sufficient elements for its full extension, or the display of its full power.

"It is this preliminary phase of the malady, wherein the vitality has been, so to speak, only grazed, and the stomach does not yet refuse to receive and absorb the medicaments, which offers to the curative art its true field for the development of its power, and it is especially concerning the treatment of this pre-

monitory phase that I ask the Academy to permit me to expose briefly the result of my observations.

"The ordinary practice, in these cases, consists in prescribing repose, diet, the use of warm and aromatic drinks, some diaphoretics, and in the last place bismuth, or opium, either pure or in the form of Dover's powder; but where the epidemic influence has become very decided, the employment of these means is far from being followed by success, and the success, when it is obtained, has often little duration; I have so often seen cholerine pass into cholera during the treatment by opiates, that in cholera times I cannot overcome the dread of opium.

"Besides, when this medicature arrests the dejections, the stomach frequently remains embarrassed, and the patient feels no return of strength or appetite.

"On account of circumstances such as I have described, finding myself, at the close of the epidemic in 1849, at the end of ordinary resources, I had recourse to a new and totally different medicine, which was mentioned in a letter addressed July 7th to the *Gazette Medicale*, of which letter I ask permission to reproduce a brief passage: 'In the last half of the month of June, when I was no longer charged with cholera service, it happened to me to receive, on the 13th and 14th, seven men who had diarrhœa, some of four, and some of eight hours' duration. According to my habitual practice, I gave them an emetic (of two grammes of ipecac.), and potions of two grammes of laudanum, also amylaceous and opiate lavements; but so far from seeing any amelioration follow this treatment, which had always succeeded with me in ordinary times, I must confess to an alarming aggravation; to the alvine dejections which were frequent, vomiting was added; the evacuations assumed the choleraic character; the voice began to grow feeble and to diminish; the pulse became almost imperceptible, and the characteristic alteration of the face left no doubt of the nature of the affection.

"'This is one of those forms of cholera often met with among feeble subjects, at the commencement or the end of epidemics.

"'I immediately placed all these patients upon the use of *mineral lemonade* (giving them a double dose of acid), and suppressed all other medicines.

"'The effect was most striking; the very next day the countenances were ameliorated, the dejections were diminished, the skin became warm, and I found in place of a slender and almost imperceptible pulse, one well developed and resistant, announcing a remarkable return of vitality. Three of these patients have left the hospital, and the other four eat from a half to three quarters of their allowance.'

"This I wrote in July, 1849. Since that time, in the visitation of cholera of 1853 and 1854, I have been enabled to apply to a much larger extent, in both the prodromic diarrhœa and grave

cholera, the method of treatment of which in 1849 I had made but an insufficient essay.

"The success, so far as cholerine is concerned, has surpassed my expectations; the diarrhœas, accompanied or not by vomitings, being arrested and cured with a promptitude altogether surprising. One may see, so to speak, the pulse rising, the skin becoming warm, the strength and appetite returning at the same time, and in a few days the patients finding themselves in a condition to return to duty.

"This result was so manifest, that all the poor consumptives of my wards importuned for the same prescription of *mineral lemonade*, hoping from it the same efficacy for their colliquative diarrhœas.

"Later still, I have had occasion to prove the infallible fidelity of this simple means, and my most ardent wish is to see it in general use.

"I earnestly implore my honorable colleagues who hear me, not to regard me as influenced by an unreasonable enthusiasm, which would be wrong in a practitioner of my age; that they will suspend their judgment on the subject; the occasions for experimentation are not wanting at this moment, and I fear that they will become too numerous.

"Two, three, at most four *grammes** of sulphuric acid with a thousand grammes of water, or a mucilaginous vehicle, with one hundred and fifty grammes of simple or raspberry syrup, make a drink as agreeable and innocuous as ordinary lemonade, and furnish at the same time a medicine cheap, easy of preparation, and everywhere accessible.

"And when, as I have so often proved, my colleagues will have been able to convince themselves of the marvellous rapidity with which this lemonade arrests the evacuations, raises the pulse and the nervous system, warms the skin, and gives to the patient the feeling of health, I doubt not that they will participate in the confidence with which its long usage has inspired me; as, moreover, these diarrhœas are very evidently only an attenuated expression of the epidemic influence, they will naturally come to the conclusion that a medicament so powerful against cholerine should not be indifferent in confirmed cholera.

"To repeat here the mode of my practice; in case of prodromic diarrhœa, and according to the greater or less gravity of the case, I add three, four, or at most five grammes of concentrated sulphuric acid to a kilogramme† of a sweetened decoction of salep.

"The patient takes every hour a glassful of this lemonade, and rinses his mouth two or three times after drinking it; it is seldom

* A gramme is about 15⅜ grains, troy measure.

† A thousand grammes, equal to two pounds, eight ounces, one drachm and twenty-four grains, Troy.

that four glasses are required. I permit the simultaneous use of the white wines, or of champagne; but I expressly proscribe the use of beer, brandy, and alkaline mineral waters during the epidemic.

"As to confirmed cholera, my practice is almost equally simple.

"The patient is kept in the most complete repose. Shampooing is practised only during the pain of cramps. Every half hour a glass of the lemonade (or from five to ten grammes of acid to a litre*), is administered, taking advantage of the moment immediately after vomiting. He takes, besides, at discretion, wine and ice.

"I think it useful to remark that the lemonade, which has a great power to suspend the alvine evacuations, produces a contrary effect upon the vomiting, increasing its frequency and duration; but this prolongation is not unfavorable, and is generally an indication of a happy termination."

The Academy then adjourned.

STATED MEETING, MARCH 7, 1866. DR. JAMES ANDERSON, PRESIDENT, IN THE CHAIR.

REMOVAL OF THE ENTIRE HUMERUS.

DR. J. B. CUTTER, of Newark, exhibited by invitation a patient upon whom he had operated for entire removal of the left humerus. The patient had first received an injury of the shoulder joint by a minié-ball. Three days after, the head and neck of the humerus were removed. Ten days after the operation, an abscess formed in the elbow-joint. The opening which it was then necessary to make, was not closed five months later, when Dr. C. first saw the case at the United States General Hospital, Newark. The entire shaft was then found in a state of necrosis, and the whole of the disease was removed during July of 1864. The heads of the radius and ulna were removed along with the humerus, but the tubercle of the radius was left with the insertion of the biceps muscle. The incision was made from the opening remaining after the first operation down the ulnar side of the limb to the fore-arm. The bleeding was almost entirely arrested by cold water. The lips of the wound were brought together with silver sutures and adhesive-plaster, and union by first intention took place throughout the whole extent of the wound. About three months after the operation, an apparatus was constructed for the patient by Dr. E. D. Hudson. The patient, with the apparatus, was exhibited to the Academy. He was able to lift a chair with ease, and go through some astonishing performances with his almost boneless limb. When the apparatus was removed, the

* A *litre* is a fraction over two pints.

muscles of the arm contracting, drew up the elbow towards the shoulder and gave the limb an unsightly appearance. The Doctor exhibited, in connexion with the apparatus, an addition which had been made to it by Dr. Sayre, which consisted of an India-rubber glove which passed over the dorsum of the hand to the ends of the fingers, and served to overcome the undue tendency to contraction which existed in the flexor muscles.

DISCUSSION ON CHOLERA (CONTINUED).

Dr. Adams asked information with reference to the utility of Chapman's ice-bag in the treatment of cholera.

Dr. Bulkley remarked that the principle had been tried in Paris during the cholera of 1832, but no very satisfactory results had been obtained.

Dr. Sayre next followed with some remarks concerning the portability of cholera poison. He maintained the doctrine of portability and *portability alone*, with much earnestness, and cited many instances, some of which had come under his own observation on board the Atalanta, in support of his position. He exhibited Dr. Marsden's plan for quarantine, and endorsed that gentleman's views in reference to the power of communicating itself which the disease possessed. The theory of Max Pettenkofer was then dwelt upon at some length, and was considered by the speaker as being the only tenable one which he had met with in all the literature upon the subject. In conclusion, he detailed the steps taken on board the Atalanta, and claimed that by the prompt and effectual means that were used, the city of New York was spared from the spread of the disease. Taking the ground that the disease could be carried from place to place, he naturally was in favor of enforcing the strictest quarantine not only for the port of New York, but for every port in the country.

Remarks were made by Drs. Harris, Draper, and Herzog, after which the Academy adjourned.

Stated Meeting, March 21, 1866.—Dr. A. C. Post, Vice-President, in the Chair.

DISCUSSION ON CHOLERA (CONTINUED).

Before the discussion upon cholera was commenced, Mr. T. McElroy exhibited, by invitation, a very ingenious model of an operating table, and also a model for a surgical bed, and detailed the workings of each.

Dr. Herzog then proceeded to lay before the Academy at great length the facts which had been collected by the Bavarian Com-

mission, which had been appointed to investigate the causes of the propagation of cholera in the year 1849. After illustrating the fact, that winds, tides, drinking-water, and seasons, had no special influence in propagating the disease, he narrowed the causes down to the stand-point taken by Pettenkofer, who, by the way, was one of the members of the Commission. He then explained very minutely the views of Pettenkofer in reference to the mode in which the disease must spread; and with that observer, in the absence of all proof to the contrary, believed that cholera must always propagate itself when the germ of the disease meets with a peculiar condition of soil which is favorable for its development. This condition of the soil he described as light, sandy, and permeable, and maintained that it must always be found where cholera exists. He also held that persons living on hard, impermeable, and rocky soils, were entirely free from the disease, and that they would escape when the disease was prevailing all around them.

He stated that it was considered more dangerous to live upon a light soil during an epidemic, than to come in actual contact with the disease. The observations of the Commission were very conclusive in settling the point, that it was extremely dangerous to wash the clothes of persons who had died of cholera. An interesting fact was also mentioned, which certainly tends to explain the reason why the disease breaks out in a given house in a locality, where no apparent means of communication has been known to exist between the person affected and another who had the disease. The cause and effect were invariably found to be associated in this way. A person who was affected with ordinary cholerine, would stay in the house for some time without informing any one of his trouble, and going off, would leave the germs behind him, which, meeting with a soil favorable for its development, would spread the disease to the inmates, who in turn would doubtless have *bona fide* attacks of cholera.

During the course of his remarks the Doctor read extracts from several German monographs which bore upon the points already referred to, and concluded by thanking the Academy for the patient manner in which they had listened to him, at the same time apologizing for the want of that thorough command of English which would enable him to do full justice to the subject.

Dr. Hutchison, of Brooklyn, then said:—It is with a good deal of reluctance that I arise to occupy the time of the Academy, especially when I see opposite me Professor Clark, who is so much more competent than I to interest this body upon the subject now under discussion. I have attended now two meetings of this Academy, when cholera has been under discussion. I came to hear some suggestions with reference to the treatment of cholera, but I have so far been very much disappointed. Very little was said upon that subject at the first meeting, and I believe little or nothing with reference to it was said at the last meeting.

I have seen two epidemics of cholera; the first during 1849 in the Mississippi Valley, where it prevailed, as the members well know, to a very alarming extent, and the second I saw here.

I am free to confess, sir, that the most valuable experience that I have derived from these epidemics is, that cholera patients should be disturbed by medicines as little as possible. I have seen them react from the profoundest collapse without the use of ice, beef-tea, or external applications. I have seen a number, too, react with the use of ice and beef-tea. I believe that in any given case where the usual remedies fail in producing any effect, it is best to rely on the *vis medicatrix naturæ*. I adopted bleeding with a good deal of reluctance, but when I did bleed I became satisfied of its effects. I have found emetics to be of great service in controlling the vomiting. I have seen patients rapidly improve after taking an emetic dose of salt, mustard, or both combined, and have seen them discharge large quantities of undigested food which had been detained for twelve, fifteen, or even twenty hours, and the vomiting then subside. I have known of one case in which the patient vomited for several hours, and when all the usual remedies were unavailable to control it, an emetic discharged a lemon-peel and the symptom immediately disappeared.

The vomiting in cholera is not attended with a vigorous effort, but seems to be more of a simple regurgitation of the contents of the stomach. Vigorous vomiting is induced by the emetic and the regurgitation ceases. When the administration of salt or mustard or both does not arrest vomiting, I believe that nothing else will be efficient.

After vomiting has been arrested, I have given small doses of calomel, one grain every hour, for the purpose of producing biliary evacuations, and have not unfrequently succeeded in producing the desired effect in the course of twelve or fifteen hours.

For the cramps, I found that the most efficient remedy was the forcible extension of the affected muscles. The cramps will then generally subside at once; and if tension is kept up for some time they will not probably return.

An observation which I also made, and which agrees with the opinion expressed by authorities upon this subject, was, that those patients who vomited and purged most freely, provided the discharges were involuntary, did best, and a favorable prognosis could be given. This led me to adopt the theory that the vomiting and purging were efforts of nature to rid the system of *materies morbi*. This theory, it will be remembered, has been held for a long time by others.

In the first epidemic I administered stimulants and opiates abundantly, and also continued the practice somewhat in the commencement of the epidemic of 1854. I soon, however, abandoned it, and became convinced that it did no good, and probably did positive harm.

In five cases I practised transfusion, or rather infusion, and the

immediate effects of this treatment were so striking that I cannot resist the conviction, that we may yet obtain more satisfactory results from its employment than we have heretofore been able to. I have seen a patient in the profoundest collapse, with cold surface, sighing respiration, profuse perspiration, husky voice, and pulseless, immediately aroused from this state, his intelligence returning with the warmth to the surface, and the profuse sweating arrested, and the fatal issue protracted. In all the cases in which infusion was employed the patient died, as the remedy was not resorted to until the patient must have died in any event.

There are a good many things connected with the subject of infusion yet to be learned. In the first place, it is a nice operation; and secondly, we not only have to learn what is the best material to use, but having found that, we have to ascertain what is its proper specific gravity, the required temperature, as well as the best method of performing the operation. I cannot doubt, sir, that when all these points are definitively settled, we may yet have more satisfactory results attending the employment of such a treatment.

I used in some a drachm of alcohol to three drachms of common salt in a pint of water, and I injected from one to two pints into the median basilic vein at the temperature of 100°—115°. I subsequently used the solution recommended by Dr. Gull, which consists of chloride of sodium, twenty parts; chloride of potassium, six parts (by weight); phosphate of soda, three parts; carbonate of soda, forty parts; 140 grains of this are dissolved in forty ounces of water, and injected at the temperature of 100°—115°.

Dr. Herzog remarked that he had seen twelve cases in which infusion was tried, but not one recovered.

In answer to a question from one of the members, Dr. Herzog stated that both sulphate of zinc and sulphate of copper had been used with great benefit as disinfectants. They had been used in solutions of one part of either salt to ten parts of water, and poured over all the materials which were supposed to harbor the poison.

Dr. Bulkley remarked that Chevallier spoke of the *sulphates* of *iron* or *zinc* as the most efficacious and economical.

Dr. Harris followed with some remarks upon the results of an experience which Dr. Vaché and himself had had with cholera at the quarantine. During his connexion with the hospital they had ten or twelve visitations of cholera, which were entirely confined to their own observation, never escaping beyond an area of twenty-five acres. The circumstances attending the invasion of the disease were quite uniform, and it was noticed that first cases showed themselves generally within three days after the arrival of passengers voiding choleraic stools. In the first few cases, or until such time as a complement of mortality was reached, all treatment was of no avail. The most fatal cases were in those who were attacked with the disease while convalescing from

typhoid fever and small-pox. He believed that the poison was communicated from person to person by some unknown agency. No such direct relation could be made out to exist between the cause and effect as had been referred to by Dr. Herzog, as the greatest vigilance was constantly exercised to prevent all communication, direct or indirect, between those having the disease and those who were free from it. The wards were widely separated, and all communication during day or night was entirely cut off, and yet the disease would spread. He was not inclined to favor any particular plan of treatment. He had used very large quantities of sulphate of iron, and the salt seemed to be a very valuable disinfectant.

Dr. Herzog, in connexion with Dr. Harris's remarks upon the fatality of the disease, stated that it had been settled that the cholera always spends itself in any one locality in twenty-five days.

Dr. F. H. Hamilton stated that he had been through the two last epidemics of cholera in Buffalo. In reference to treatment, he believed very little could be said to encourage any particular method. He had proved to his satisfaction that all remedies availed little or nothing during collapse. Very few indeed recovered from that condition. Stimulants and opiates had uniformly failed in his hands. He knew of one man who recovered after a full dose of calomel, and two after the administration of emetics. He referred to the case of one man who fortunately was left to himself, and being near a pail of water, drank himself well. He had given fifty grains of quinine to one patient within three or four hours, but a fatal result was not averted.

There was one point of value in connexion with treatment which he thought had been overlooked, and that was the advantage of change of locality. In proof of this he cited several instances, the most striking one of which was the following: During the last epidemic in Buffalo, the cholera was raging in a sort of excavation near Suspension Bridge. No remedies were of any avail. The people were dying with great rapidity, and the aspect of affairs was indeed desperate. Finally, Dr. H. concluded that the only way to save those who inhabited that particular little street was to clear them out of it, and get them in a more elevated situation. This he succeeded in doing, by the help of the Mayor, before the dawn of day, and as soon as it was done the fatality ceased. Only one died after the change was effected.

Dr. Stiles stated that during the cholera of '54 he was assistant physician to Kings County Hospital under Dr. Turner. The hospital contained from 400 to 500 patients, and in its immediate vicinity there was a poor-house and insane asylum containing as many more. There were in all about 1,000 patients from whom cholera selected its victims. The number of cases affected was large. This was probably caused by the existence of a lake within 200 yards of the hospital, which received all the drainage

from the institution. Certain it was, that whenever the wind was blowing from the lake towards the hospital, the cases of cholera would be more numerous and more fatal.

He stated that he had risen more particularly to call attention to one or two facts, which he considered of importance to note in connexion with the treatment of the disease. Several times on tying up the arm, in order to practise the injection of the saline fluid, the patients would suddenly faint and die. This became so frequently attendant upon making any attempt at infusion, that the patients naturally refused to have the operation performed. Indeed it was never attempted after awhile, until all hope of recovery was supposed to be passed. All the patients who had infusion practised upon them died. Taking a hint from the result of tying up the arms of patients, it was thought best to make use of frictions of the extremities in the direction of the venous circulation, in the hopes that life, by that means, might be sufficiently prolonged to allow the disease to spend its force. The only cases of recovery that took place during the first part of the epidemic were seemingly due to the employment of that method of treatment. In conclusion, he called attention to the danger of applying heat over 100°, and also to the futility of attempting to cool the spine by ice-bags when a current of warm blood was constantly circulating in the vicinity.

Dr. Joel Foster gave the history of his experience during the cholera of 1832, he being then a practising physician in Schoharie. He related several cases that seemed to be cured, even from collapse, by the use of large doses of calomel and hot enemata.

On motion of Dr. White the subject for discussion was continued, when the Academy adjourned.

Stated Meeting, April 4, 1866. Dr. James Anderson, President, in the Chair.

DISCUSSION ON CHOLERA (CONTINUED).

Dr. A. Clark exhibited some plates of Pirogoff and Cruveilhier illustrating the pathological anatomy of cholera. He desired merely to exhibit them to the members, at the same time remarking that all that he knew about the subject of cholera could be found in the recent numbers of the *Medical Record*. This being the case, he had no desire to make any unnecessary repetitions; still he thought it proper, in connexion with the plates, to remark upon them. He remarked that whatever might be the origin of the choleraic action in the body, the first great prominent fact was an extraordinary congestion of the alimentary canal, either in the whole or part of its extent. He then exhibited in turn different plates which illustrated the existence of the following: As far

as the stomach was concerned it was seen to be occasionally contracted, the mucous membrane pale or studded with a punctate injection, the solitary glands enlarged, and not unfrequently spots of ecchymosis were found scattered over the mucous surface. Nearly the same appearances, more or less modified, were to be seen in connexion with the small and large intestines. Occasionally the mucous membrane could be seen in a sloughy condition, and not unfrequently not only the solitary but the Peyerian glands were enlarged; and again, there could be seen exudation upon the membrane of a granular-looking material, which very much resembled that seen in diphtheria. Still again, the mesenteric glands were not unfrequently found enlarged.

In reference to the condition of the lungs, he remarked that it would be desirable, in view of Dr. Geo. Johnson's theory, to acquire as definite an idea as possible of the quantity of blood found in the organs of persons who die during the stage of collapse. He stated that Professor Geo. Johnson, of London, holds that the lungs, under such circumstances, contain much less than the usual quantity of blood; and that the right cavities of the heart are filled with blood. This latter fact had been long enough acknowledged; but with regard to the anæmic condition of the lung, he was not aware that enough attention had been drawn to it to substantiate the views of Dr. Johnson. It was a subject that was well worth the study of pathologists, and its importance required that more extended observations should be made before the fact could be firmly established. As far as the injection of saline materials into the veins was concerned, he wished to offer the suggestion that, in view of the fact that the saline elements in the circulating fluid were not much, if any, decreased, but, on the contrary, were increased sometimes in quantity, a beneficial effect may be gained by the infusion of simple warm water.

Dr. Bibbins said:—During the cholera of 1849, I was House Physician to Bellevue Hospital. No cases of cholera were admitted into the institution; but if a case occurred among the patients it was suffered to remain for treatment. The strictest medical police was enforced to prevent the occurrence of cases within the hospital. If a case of diarrhœa occurred it was at once reported to the physician, who gave his immediate attention to it. Every effort was made to cure the diarrhœa without delay. The result of all this was that very few cases of cholera occurred in the building during the summer.

One night, between nine and ten o'clock, a man was brought in with a badly crushed leg. He had received the injury at the Franklin Forge, situated on the opposite side of Twenty-sixth street, by his limb being crushed by the trip-hammer. An amputation of the limb was rendered necessary within the large surgical ward, where the patient was first carried.

All the other patients from the surgical ward stood and saw the operation. Nine of these suffered from diarrhœa before morning,

and one was attacked with cholera. This fact is an important one, as showing the depressing influence which the operation had upon the system of this particular individual. If the inference is correct, it shows the importance of counteracting all depressing influences, even that of fear, upon the people.

While cholera was prevailing there, Dr. Gilman asked me concerning the treatment. In his peculiar way he told me of his experience in connexion with the cholera hospital during the epidemic of 1832. He said that he, in company with another, had charge of a hospital, and that at the end of the season when the hospitals were about to be closed they made up their returns, he found that his hospital returns were the best. "The reason," said he, pointing to his forehead, "was here. The others treated their cases; we did not treat ours." He was to be understood as meaning that they used principally the expectant treatment in larger degree than the other.

One point I would state, and that is this: Dr. Reese was then resident physician in Bellevue, and being in favor of blood-letting, that remedy was tried. All the cases in which this was resorted to were unsuccessful.

Dr. Herzog thought that in speaking of the treatment of cholera, members should be particular to name the particular stage of the disease in which any particular remedy was resorted to. The questions in regard to the utility of certain remedies should be: I. Which remedies have been most successful in arresting the diarrhœa? II. Which have been most useful in arresting rice-water discharges? and, III. Which have been most serviceable in preventing collapse?

Absolute quiet alone had prevented eighty-six per cent. of the cases of diarrhœa from passing into cholera proper, while opium had accomplished the same thing in eighty-seven per cent. Large doses of calomel seemed to have a favorable influence in arresting the rice-water discharges. This remedy was given in doses of from ten to fifteen grains, every hour or two.

Dr. Batchelder remarked that he experienced great benefit from the use of opium and acetate of lead in the first stage.

Dr. Vandeveer stated that he had treated half a dozen cases with blood-letting, and they all died. In other cases where chloroform was used to allay spasm, the result was alike unfavorable. His most reliable treatment was that by calomel, sixty grains for first dose, and thirty grains when the remedy required repetition. He persisted in administering it when vomiting existed until he was satisfied that enough remained on the stomach to do good. During '49 he used opium with great benefit.

In conclusion, he stated that he opened seventy-five cases. In some there were marked changes in the brain, in others equally marked changes in the spinal marrow. There was most generally an effusion of serum into the cavities of the membrane of these

centres, as well as in the lateral ventricles of the brain. The mucous membrane of the stomach was almost invariably softened, so that it could easily be scraped up with the nail.

Dr. Gunn stated that he was one of the assistants in the Greenwich Cholera Hospital. Out of 650 patients treated, 222 died. Every variety of treatment was tried; as for instance, calomel and opium in large doses, and frictions with the camphorated mercurial ointment. But these like all others failed, and in several of those cases in which the frictions were resorted to, the patient died with the most violent ptyalism. In reference to the infusion of saline mixtures into the veins, he recollected five cases which were subjected to that treatment in one afternoon, and all died in two or three hours. Subsequently his associate, Dr. James Clark, tried the injection of tepid water in the veins, and was in a measure successful.

Finally, as the result of a good deal of experience and observation in the treatment of the cases in that institution in '32, calomel, opium, and camphor, one grain each, was looked upon with favor. This remedy was employed in conjunction with ice internally, and heat externally. In 1834 about the same treatment was followed. If the cases were seen before collapse had taken place a large proportion got well.

In 1849 and 1854 Dr. Gunn remarked that he tried in his private practice another plan of treatment. He used a combination which he styled the camphor mixture. It consisted of nitric acid 10 drops, tr. opii. ʒ i., aq. camph. ℥ iv. The dose was a dessert-spoonful every hour or two. Strong mustard poultices were placed over the bowels, and to the extremities red pepper was applied. Ice was administered internally. If there was any disposition to collapse, he used small lumps of unslacked lime enveloped in wet muslin. These were placed under the axillæ or between the thighs. The patient was then covered up with blankets, and in ten or fifteen minutes after, perspiration would start out and the vomiting would cease. He spoke in the highest terms of the success of the lime treatment, and believed it ought to be resorted to in all cases of collapse. He had not been in the habit of using calomel internally until reaction had been established, and then the dose was not more than twenty grains.

The Academy then adjourned.

Stated Meeting, April 18, 1866. Dr. James Anderson, President, in the Chair.

Discussion on Cholera.—(Continued.)

Dr. W. C. Roberts read a paper on the infectio-contagiousness of Asiatic (Epidemic) Cholera.

The length of the article precludes its publication in full in the

Bulletin, and compels us to give only an epitome of its more prominent features.

Dr. R. began by alluding to the importance of the question in reference to Quarantine regulations and measures of Sanitary reform and precaution connected with the public health—matters which were never more vitally momentous than at the present time. The disease was awakening from its slumber of sixteen years. It had been, during the past few months, at Gibraltar, Nice, Brest, Montpelier, Lyons, Paris, etc., in England, on the Rhine, on the Island of Guadaloupe and elsewhere, and still existed. It had been at our Quarantine station within a few months, when it was prevented from extending by the vigilance, intelligence, and promptitude of action of our officials. It is now at Halifax on board of a steamer crowded with emigrants on their way hither from Germany, *via* England.* If it extend by atmospheric agency *alone*, no human means can prevent its introduction and spread among us. It will pass over all barriers and cordons. But, if it can be imported from abroad; if Quarantines have not *always* failed to prevent its introduction into uninfected ports; if *another element* than mere atmospheric infection has a propagating influence; and if the cholera obeys the same laws as all other epidemics originating in a *specific* poison, then must those means which an enlightened science suggests, and long and careful experience dictates, be fully and firmly enforced, and cannot fail to be useful.

"I readily acknowledge," says Dr. R., "the introduction of disease by atmospheric transmission, and the inability of Quarantine to exclude it; but it rarely comes to us in this way. I acknowledge also the powerful influence of bad drainage, defective ventilation, overcrowding, offensive sewers, unwholesome water, and the thousand other kindred abominations which afflict the poor, in the *extension* of disease, although they don't generate it. But I believe also in the importability of the seeds of specific infection, which, amid predisposed persons and in predisposed places, produce, reproduce, and disseminate endemic, epidemic, and pestilential diseases; and I would rigidly exclude their admission, either by infected persons or families, as far as possible. That this can be done, has been done, is doing, often with success, the records of the past as well as of the present time abundantly testify.

"Cholera is not indigenous here, even with filthiest and most likely localities. It is doubtful whether now, even, it has even a sporadic origin here,† and I might even say elsewhere, except in its original habitat. It travels, it is imported, brought, introduced from abroad, and almost always by way of the sea.

* Several other infected vessels have since arrived here, and are still at Quarantine in our harbor, and many cases occur daily on board.

† It is said to be always present in Calcutta.

Whether it is always present, or originates at intervals *de novo*, at the seat of its origin in India, is unknown. Upon the nature and contagion of typhus, small-pox, scarlatina, there exists no doubt. It is quite an unsettled point, and until the nature and laws of its poison are agreed on, always will be. Not so as to cholera. It is a *specific* disease. Has it a *specific poison?* Is it now an animal one? Is it *in any way* communicable from the sick to the well? Upon the settlement of this grave question depends the health of nations and of individuals."

"All true epidemics originated in infection and are infectious; the first cases were certainly such; all probably at origin, however, possessed that capability of intercorporeal zymosis which rendered them contagious. They may again originate without contagion. They are specific, result from a specific poison, produce specific diseases, reproduce themselves within the body, and therefore are necessarily infectio-contagious. The degree of this communicability, which depends on several circumstances, differs. None, perhaps, are always dependent on contagion, cholera least of all. Their laws vary but little. What is true of each or any of them, is true in a degree of them all. Once produced, they become *animal* poisons, which are all specific contagions capable of specific reproduction. It would be strange if poisons which possess this power of acting upon and so deeply disorganizing the blood and nervous system, and of infecting the secretions and excretions, should lack this animal specific *zyma* which gives them their contagious property." None of them, in fact, do want it in a certain degree.

The great questions connected with the subject of the importation and extension of epidemic cholera are four.

1st. How does it reach us from abroad? By means of atmospheric transit and diffusion? By ships, infected persons, or fomites?

2d. What is the probable nature of the poison?

3d. What are its habitudes, laws, and mode of progress when prevailing?

4th. Can quarantines exclude, or sanitary regulations wholly or in part lessen its virulence as a poison and diminish its spread and prevalence as an epidemic? And if so, how and what are the measures proper to be adopted?

Cholera is the one great epidemic. None of the diseases which have at different times ravaged the earth have equalled it in extent, rapidity, and simultaneousness of progress, nor in mortality. It is not a new disease nor a new poison. How or when it originated is unknown; but it had existed in India for centuries, perhaps, before its great outbreak in Jessore, in 1817. It began as a zymotic endemic, and became epidemic. It has probably never since been extinct in that country. It first spread into Europe in 1830, and reached us for the first time in 1832. We have had it since in 1834, in 1849, and 1854. Dr. R.

here sketched the history of its progress in Europe and America in 1832, and detailed its vagaries of route, course, speed, progress, etc.

He then submitted the following deductions from the elaborate and generally candid and well-reasoned, though not always quite consistent, "Report on the Causes and Diffusion of Epidemic Cholera," by W. Baly, M.D., drawn up at the desire of the Cholera Committee of the Royal College of Surgeons, London, after the epidemic of 1832.

1. The fact is amply confirmed of the dissemination of cholera through a country after its importation, by human intercourse (p. 190); in other cases it seems also to be shown that no such cause can be traced or probably existed.

2. The cause of cholera is a virus *reproduced in the bodies of the sick*, or a poison which undergoes its increase *entirely in the atmosphere*, or, at all events, out of the human organism, not dependent on generic atmospheric influence (214); or a material substance, only partially distributed, and rife only under certain defective conditions of site and sanitary provision, affecting the condition of the air in the places in which they exist (216). Its cause, it is further inferred, from the very local and persistent character of the outbreaks of cholera, is not a gaseous diffusible substance, but rather matter in the form of solid or liquid particles, capable of attaching themselves to surfaces of other bodies; atmospheric currents, most probably, concurring with human intercourse in the office of disseminating cholera, and rather over limited areas than to distant places, "which is probably effected in a majority of cases by the locomotion of men" (220). "The frequent communication of the disease (222) by clothes or bedding of the sick, to the persons who handle and wash them, under circumstances rendering other sources of infection than emanations received from the bodies of the sick impossible, appears to be by no means proved (?)" (212). "On the other hand, the evidence respecting the *especial* liability of nurses and others attending on cholera cases is, in some instances, of such a character as to preclude the absolute rejection of the idea that this disease has a contagious property, even though it *does not generally spread* by virtue of contagion." "The largest amount of evidence," they say, "supports the view, that the cause of cholera is a matter increasing by some process, whether chemical or organic, in impure damp air," and assumes that, and although of course diffused with the air, it is also distributed and diffused by means of human intercourse."

This can be effected only in two ways: by fomites and by the infection of the person of the sick, which *is contagion*. "The fevers, diarrhœa, etc., which often precede and accompany cholera, are probably caused by different poisons, all of which find their means of increase in more or less similar states of the atmosphere, and are, therefore, although coëxistent, probably *neither choleroid nor premonitory*" (224).

The last passage I shall quote has a deep significance. "The possibility," says Drs. B. and G., "that cholera is occasionally communicated by a virus produced in and emanating from the sick, has been (by us, understood) admitted."

If this view of the subject, which has, as will be shown, the sanction of some very eminent authorities, be once admitted, the occasional contagiousness of cholera can no longer be denied. The term "occasionally" applies rather to the susceptibility of the person exposed than to the nature of the infecting cause. No contagious principle always infects every one with whom it comes in contact. None is equally virulent at all times. They are most so at the beginning of an epidemic; they are weakened by atmospheric dilution, by extent and duration, and finally cease to operate, partly from lack of intensity on their own part, and partly, perhaps chiefly, from the want of longer susceptibility and predisposition of persons and places to epidemic influences. But they are, perhaps, never extinct.

Cholera spreads more by epidemic influence than typhus, small-pox, or scarlatina; but if its contagiousness can be shown, in one single instance only, with the same clearness and certainty as in these recognised contagions, that one instance proves the contagiousness of the disease as well as a thousand. Once contagious, it is always so; but not equally so; nor is it claimed, so far as I know, that cholera is *always, or even oftenest*, propagated by contagion, personal or fomitic.

If it can be shown conclusively, by irrefutable examples, susceptible of no more rational explanation, and such as would suffice to establish the contagiousness of other contagious poisons, that, by the introduction of infected fomites, or of a person or persons sick or in good health, coming from an infected locality, the disease was introduced into and engendered in a healthy town or village; that the poison of the disease came in with these fomites or persons, and from them extended to other persons in the same village or town, and thence, along the lines of and by means of human intercourse, extended to other places and infected them with a specific disease resulting from a specific poison, then the evidence of the contagiousness of cholera is as strong and sure as that of small-pox itself, although perhaps "only in a modified form, and not as typhus and small-pox are." The reader, on the basis of the arguments submitted as to the laws and nature of specific poisons generally, must decide for himself how far even this modification can be accepted.

Cholera is propagated, as I suppose, in one of four ways.

First.—By its extension from its original seat of generation in India, whence it always seems to issue; thence, subsequently, from country to country and place to place, either by the medium of atmospheric currents, or by some inherent peculiar power of progression of the original specific poison, whether that be solid, liquid, or gaseous. This I take to be simply infection.

Second.—By the simple infection of fomites, filled with the morbific air of infected places, which, opened and exposed, as on board ship, for instance, spreads the disease.

Third.—By means of fomites infected by the excretions of the sick, or even by the excretions themselves—the discharges from the stomach and bowels especially; which, as I suppose them to contain the specific poison of the disease reproduced in the bodies of the sick (else how do they become morbific?) I look upon as specific contagions.

Fourth.—I assume that it is communicated to the well by indirect contact with the sick; their bodies reproducing in themselves, by a zymotic process, the elements of disease, which are afterwards eliminated, or emanate and become infectious, in a manner similar, as I suppose, but less actively in degree, to the morbid poisons of *the* other contagious epidemics.

M. Moreau de Jonnes, in a recent report to the Supreme Council of Health of France, denies, I see, the atmospheric current doctrine, and claims that cholera is communicated to the world only and solely by direct or indirect contagion. Difficult as it may be to explain the spread of cholera, in many cases, by the atmospheric current doctrine alone, there are at least as many in which it would be difficult, if not impossible, to do so, without assuming it to be true; and it is not generally denied. Cholera certainly is a morbid poison. People are simultaneously and progressively affected by it. Now, if it had not a spontaneous origin of local or atmospheric character, or depended wholly upon atmospheric transmission, I see nothing in the manner of its infecting that differs from that in which small-pox and measles infect, nor any reason for doubting its equally contagious nature in certain cases. There are plenty of seeming facts to establish this view; and if they be facts, all the reasoning in the world will not invalidate them.

Four of the ablest physicians of the present day, who have investigated the subject of the contagiousness of epidemic cholera, are Dr. Watson and Copland, of London, Simpson of Edinburgh, and Graves of Dublin.

Dr. Watson (Lecture on Principles and Practice of Physic) thinks that "the complaint, in every instance, was excited by the application of some noxious material, some positive poison, to the body; its violence of action differing according to the susceptibility of the exposed." "The poison travelled, was portable, and therefore communicable from person to person." "Whether the cholera poison had the zymotic power of multiplying and reproducing itself in the human body, is more doubtful." "That it is contagious, in a limited sense, but that its contagious power is not very great;" "that a great majority of the cases were not attributable to direct contagion, but to the poison diffused through the atmosphere;" "that the noxious matter travelled sometimes by *its own peculiar powers*, sometimes made use of

vehicles;" "that poverty, which implies other evils, was one of the strongest predisponents."

Dr. Simpson (Edinburgh *Medical Journal*, vol. 49, "On the Evidence of the *Occasional* Contagious Propagation of Malarious Cholera") is of opinion that cholera, *like small-pox*, is only *occasionally contagious;* and as, before 1817, it never spread epidemically over the world, it is not philosophically referable to common exciting causes of disease, inasmuch as they never give rise to specific diseases. It is therefore in its wide-spread epidemic form, at least, specifically new, depending on new and specific agency, or agencies operating on the human body only when and where it prevails." "It is too erratic and diffuse in its course to be logically traced to the action of causes having only a limited and local existence."

"Its spread and diffusion, as of general epidemic diseases, may arise from three morbific causes. 1. A peculiar influence, property, or miasm, existing in the atmosphere; 2. An influence emanating from the earth; 3. A contagious influence generated in the human body, while laboring under the disease, capable of exciting the same specific disease in such predisposed individuals as are exposed to its action by direct or indirect intercourse with the infected." "Contagion is the transmission, by *specific poisons, of specific diseases*, from the sick to the healthy, both by direct contact and by the more indirect medium of air, or of *other* fomites, impregnated with the morbific effluvia *generated in the bodies of those affected;*" a definition, said Dr. R., which needs to be borne closely in mind by any who desire to arrive at correct conclusions in relation to this or any other of the epidemic contagions.

"Cholera," adds Simpson, "*does not always spread by contagion*, but having once admitted the evidence of the latter to be complete in some instances, others in which this cannot be traced or demonstrated, are merely negative." "While its propagation and march do not depend upon variations in the extreme meteorological and physical agencies to which the human system is exposed, yet it cannot be doubted that a peculiar general epidemic constitution is liable to prevail in certain localities and at certain periods, which, by its collateral influence, tends to promote in a great degree, the contagious diffusion of the disease; determining by its presence the particular severity of the malady in some localities, and by its absence rendering the contagion when imported, comparatively innocuous in its effects, and limited in its power of propagation;" "epidemic constitutions equally essential to the full prevalence and spread of continued fever, scarlatina, measles, small-pox, etc., have their greater dissemination during some years than in others."

"The occurrence of cholera in new and isolated localities, not traceable to direct communication with infection, its confinement to particular streets, houses, etc., its not invariably infecting the

exposed, is as true of small-pox as of cholera; and the contagious character of the one, admittedly such, is no more invalidated thereby than that of the other."

"Individual predisposition, physical or personal, is absolutely necessary for, and essential to the successful action of morbific agencies on the human system; and, as the circumstances of no two individuals can ever be precisely the same, it is to this want, or difference, of predisposition on the part of some, and not to the non-contagiousness of the cholera, as well as other diseases, that we are to ascribe the escape of persons exposed to direct contact with infection. Yellow fever, small-pox, and syphilis are not *always* communicated by direct exposure, nor even by direct inoculation." For the examples by which Dr. Simpson illustrates his doctrine, the original paper must be consulted.

Dr. Graves, of Dublin (papers in Dublin *Medical Journal*; February and May, 1849, at the beginning of the epidemic of that year in England), comes next under review. The following is a brief summary of his positions.

He fully believes in the importation of cholera and its contagiousness. He approves of cholera hospitals. He opposes the idea, that sewers, streams, rivers, and damp localities, which have existed for ages, can engender, in any or different seasons, specific diseases, cholera especially, which he terms "new and specific," or can now be safely invoked to explain its origin. Means of increasing the comforts of the poor he deems superior in importance, as preventive of disease, to mere cleanliness. "Neither has it," he says, "been shown that the state of the general health, for some time previous to the outbreak of cholera, has always been such as to indicate a state of air necessarily precursory of that disease; the accurate reports of the Registrar-General having proved that the public health was never better than it had been for some weeks previously." "It is equally true of scarlatina and small-pox, and was equally true of the great plague of London, in 1665, that the disease ceased in towns before the whole body of inhabitants were affected, owing to the fact that many constitutions are not susceptible at all, or very slightly; and so, when a certain number of these who were, have been attacked, the disease ceases for want of individuals whom it can attack. Such anomalies in the history of cholera are not sufficient to take it out of the category of contagions."

After alluding to the escape from cholera, in Rome, during the last epidemic there, of a filthy quarter called the Ghetto, Dr. Graves observes: "If, then, this locality was spared, can cholera be produced anywhere by those agencies on which the advocates of dirt and want of drainage as its causes so confidently rely?" Dr. G. then exhibits the liability of physicians, nurses, and others about the sick, to infection, and shows that their frequent immunity occurs equally in other confessedly contagious disorders. He spurns the idea that a filthy sewer should be supposed capable of

generating any, or successive diseases at different seasons, and especially "*a new and specific one*, cholera." For numerous instances of the liability of medical men, nurses, etc., and of contagion in its various forms, the reader is referred to the paper itself. Dr. Graves opposes quarantine, because, by closing all ports, which alone could be effectual, famine, which is worse than cholera, would be engendered, although a knowledge of its contagious nature is of immense importance. "Whatever influence poverty, and the condensation of wretchedness among the poor, may have in producing fever, gangrene, erysipelas, and dysentery, yet *they never can originate specific diseases.* The combination of no malign influences is capable of giving rise either to measles, scarlatina, small-pox, and I will add, cholera, for if it had, it would not have been of such recent date among us." "Nuisances are indeed disgusting; but the causes of epidemic diseases escape the scrutiny of both eyes and nostrils. The worst parts of most capitals of Europe, however, abounding in all sorts of abominations, do not give rise to either *typhus*, or plague, or cholera. Poverty, of which filth is the outward and visible sign, is of more injury than dirt in increasing the spread of contagious diseases; but, alas! the remedy needed will require something more expensive than the broom and brush." In concluding his admirable essay, Dr. Graves considers that the transmission of typhus to America and Canada, shows its portability and infectiousness; the single instance of the steamship *Eclair* takes yellow fever, he thinks, out of the catalogue of non-contagious diseases. The case of the Brig *Amelia*, in his opinion, decides the importability of cholera. "With respect to the Irish (typhus) ships, the cause of the mortality is now universally admitted to have been contagion; and that same cause it was that produced the disastrous spread of cholera in the American brig (Amelia)." Such is a resumé of one of the ablest existing arguments in favor of the contagiousness of cholera, and the analogy it bears to other congeneric contagions depending on specific causes, by one of the most esteemed of the "ardent men" of the profession who have devoted their abilities to its investigation, and whose minds have arrived at this conclusion.

Copland (Dictionary of Practical Medicine) is another staunch contagionist. This exhaustive article abounds in such proofs as these rely upon, and replies *in extenso* to all objections. He says: "It (cholera) was caused, in my opinion, by a healthy person *inhaling* a morbid miasm, or effluvia, *an animal poison*, which, *emanating from the body of the affected person*, contaminated the surrounding air and infected the predisposed." If this were proved, or generally admitted, there would probably be little difference of opinion left on this disputed question. Snow believes that it is owing to *a specific poison*, material, liquid, or solid, which is *swallowed*, exclusively contained in the matters purged or vomited, which must be attached to fomites, or disseminated through water, to be carried far.

"Whether it was, at its origin, an animal poison, or became such only after passing through the body; whether, or not, it has continued to propagate itself without any subsequent *de novo* generation of the poison, with only occasional lulls in its intensity and prevalence; or whether it arises in distant and unconnected places, at one or different times, and so on several and successive occasions generates the contagious principle *de novo*," are questions equally applicable to cholera as to other *specific* epidemic diseases, and cannot be answered with certainty. A tendency exists at the present day to allow to all of these diseases, typhus, erysipelas, measles, small-pox, and scarlatina, an occasional *de novo* and spontaneous origin (see J. M. Smith, Philosophy of Epidemics: Gregory, Lectures on Eruptive Fevers, 1851, p. 79). The question of contagiousness, when generated, is not however affected by the decision of this question in either way. Other arguments in favor of contagion are to be found in a pamphlet by W. Read, M.D., City Physician, of Boston, Mass., 1866, with a judicious summary of necessary quarantine and sanitary regulations, in view of future contingencies. Dr. R. magnanimously recants his former non-contagionistic views; which, when the events which have recently occurred at our own quarantine station, which occurred in 1848 in the case of the ship Atlantic, of the Atalanta in October, 1865, etc., are considered, is certainly highly judicious. What would have been then, and what would be now, the fearful consequences of allowing the passengers and baggage from these numerous infected ships to have been "permitted free communication with their friends on shore, or with the city, or landing at any time," as the "consulting physicians" of said city had previously agreed to recommend (see pamph.).

In the *New York Medical Record* for 1866, is now in course of publication a series of able lectures delivered by Prof. A. Clark. In the third of these occurs the following passage: "All recognise a special, perhaps I should say a specific poison; and indeed it seems difficult to survey the history of the disease, and not believe that it is produced by a *poison of its own*."

Speaking of the several views which have been entertained of its cause, "one," he says, "assumes that it is contagious, a communicable zymosis; the zyma ferment, or specific cholera poison, finds its way into the blood, rapidly multiplies itself, and is then diffused into the surrounding air, to be received into the bodies of other persons, and in them to be again reproduced." This we suppose to be the generally received and admitted theory of epidemic contagion, animal and specific. "Another view," says Prof. C., "recognises a zyma and zymosis *not within the body*, but in the air and soil;" like that which gives rise to yellow fever. "The poisonous air becomes, by some catalysis, a ferment, and produces its like. It also assumes the presence or existence of an epidemic constitution of the atmosphere, widely diffused over a given country, predisposing to cholera with the aid of certain

local sources of insalubrity; not generating cholera, but loading the air with *other poisons* which aid the cholera miasm in its fatal work" (Lect.).

If now these theories be combined, and it be assumed that cholera occurs as a zymotic miasm, reproducible and multiplicable *both in the air and in the body*, which is probably equally true of the other epidemic poisons, we seem to be furnished with a rational and satisfactory idea of their manner of propagation. Carried by atmospheric currents, disseminated by human intercourse, the poison, multiplying as it spreads, continues its course and prevalence, until it has affected its most predisposed victims, when its energy declines: an energy, however, not the same in all persons or places. Its progress varies in obedience to each of these modes of transmission, and its intensity and fatality vary with the "localizing" conditions as to insalubrity and predisposition of persons and places, and the medical constitution of the air.

Thus, it seems, we may account for the greater intensity of cholera in some places than in others; for its rate of progress, rapid or slow; for its turning off *en route* to attack opposite or remote localities; its tendency to advance, recede, or tarry; for the eccentricity and caprice of its erratic course: localities escaping now which in their turn may at other times be invaded, or even returned to.

It might thus happen that a ship at sea, long after having left an infected port, and without sickness on board, shall enter into or be overtaken by a wave or streak of atmosphere loaded with this choleraic zyma and poison, and thus the disease occur in her; circumstances not very unfrequent, and now attributable only to one of three possible causes, the most probable of which is the opening of infected fomites, and least so, spontaneous development or protracted incubation, which, after a certain time, say thirty days, seems impossible. This capability of spontaneous progression of multiplicable zyma, in the air, and with or without the assistance of its currents, *may be* the "one peculiar power of progression" alluded to by Dr. Watson, as already quoted; nor are these crude speculations to be deemed wholly unreasonable upon a subject which as yet only admits of plausible conjecture, supported too as they are by respectable authority. The cause of cholera is analogous to this of yellow fever in this, that it arises in certain localities, from certain causes combined of atmospheric and telluric origin, and is transported in ships beyond the seat of its origin. Yellow fever seldom extends far beyond the edges of seaports, to which it is brought in ships. But cholera extends from the seat of its origin all over the world. Both are communicable by fomites, and both are believed to be contagious; communicable, that is, from person to person—yellow fever less so than cholera; cholera less so than typhus or small-pox. Neither of them is as yet domiciliated.

"Cholera," says Dr. Clark, "is not contagious, but its cause is portable." It remains to be seen whether that cause is regarded by him in the light of a specific poison, and where he derives it. It is very usual to assert the qualified contagiousness of cholera. Dr. Burrall does this. "While," he says, "the disease is not contagious in the same manner as small-pox and scarlatina, yet the cause of connexion between its occurrence and the arrival of an infected individual in a previously healthy neighborhood, is such as to leave no doubt of its communicability." If the poison of cholera be, as I suspect it will be admitted to be, a specific, reproducible zymosis, there will not be much difference in the nature and manner of their contagiousness. The parallel with small-pox is already pretty strong.

"Small-pox," says Watson, "like the others of this group of contagious exanthemata, has its periods of dormancy and activity; its 'caprices,' as we ignorantly term them. Every now and then it overruns a country or a district as an epidemic. It began in Sweden in 1824, reached England in 1825, spread to France in 1826–7, and ceased in Italy in 1828–9. In London intervals of twenty-four, fifteen, twenty-nine, thirteen, and six years have occurred between the great epidemics of the last hundred years. It is communicable both by the air and fomites (letters, etc.). It crosses oceans and rivers. It returns to a locality after a varying lapse of years; the intervening ones presenting only just cases enough to maintain the slumbering contagion. It has, like the other allied maladies, its periods of inception, increase, culmination, and decline. Nor can it, although generally believed now to arise only from contagion, in every epidemic be traced directly to this cause. Many anomalies occur, however, which might perhaps, were the circumstances thoroughly known, be easily explained. It broke out in a prisoner in solitary confinement in the Milbank Prison; a case which is parallel to that of the negro confined for many years in the Massachusetts State Prison, in July, 1854, alluded to by Dr. Clark, in support of the non-contagiousness of cholera. These cases show that either the diseases had a spontaneous (*de novo*) origin, which is claimed for small-pox, but not generally allowed, but never yet, so far as I know, for cholera out of India; or that the poisons, in some way, entered the prisons from without, which was very possible in either case. In the case of the cholera patient, it may be said that no contagionist has ever claimed that cholera *always* originated from contagion; and secondly, that 1854 was a cholera year. The poison was abroad. Before the doctor reached his bed again four other cases were announced, and in forty-eight hours two hundred and five followed. The epidemic constitution prevailed in the locality; and these, though strangely selected, were its first manifestations. Lastly, there are cases of small-pox without rash, as there are cases of cholera without evacuations.

"The parallel is a very close one, and suffices at least to ally

the diseases. Cholera is not yet domiciliated here, and therefore does not, as is claimed for small-pox, always (*or solely*) arise from contagion; neither are the visitations of small-pox as sweeping, though capable of wide diffusion. The contagion of small-pox is more direct and virulent; but its emanations, which are supposed to be from every part of the body, extend to more limited distances beyond it, and are more volatile. Those of cholera are supposed to be chiefly from the evacuations. Cholera infests and travels further and faster than small-pox, by virtue of its own greater inherent power of progression, and its epidemic spreads are wider than when mere contagion alone is concerned.

"Nevertheless, I believe that they both arise from specific animal poisons; zymotic in the air and within the body, reproducing themselves in both; multiplying, progressing, infecting by the atmosphere and fomites, by contact with the sick (Simpson's definition), and thus, necessarily, infectio-contagious; not, however, invariably or equally in degree."—(*Watson*, p. 808.)

The paper concludes as follows: "While I am not a believer in the possibility of engendering specific diseases *de novo*, by filth, etc., I am entirely convinced of the absolute necessity of measures calculated to destroy that predisposition and susceptibility to the contraction, maintenance, and diffusion of diseases, to which personal and local uncleanliness, etc., undoubtedly give rise, and which so vastly increase their mortality; and I earnestly hope that the praiseworthy efforts in the right direction, now making by our new Board of Health, will not only be warmly supported by the profession and public, but be crowned by a triumphant success."

P. S.—The following interesting addition was made to the knowledge of the author on the subject since the paper was read:—"Fully developed cholera may be recognised as a *specific disease.*"—P. 183. "The facts relating to its appearance, etc., etc., warrant the inference that the efficient cause of the malady is not *exclusively* a contagious principle, nor exclusively an atmospheric one. To reconcile and explain the facts before us, it seems necessary to admit the existence *of both* these principles."—P. 185. "It manifestly propagates itself, in certain circumstances, by personal communication, and must be regarded as arising from contagion, but in only a minority of cases."—(*Rep. on Med. Top. & Ep. of State of N. Y. to Am. Med. Assoc., June*, 1860, by Jos. M. Smith, M.D. Phila. 1860.)

Dr. J. C. Draper presented some specimens of lead pipe lined with tin, which he thought was a great improvement upon that now in general use for Croton water. He referred to the instances of poisoning by lead which had become so frequent since the general use of the lead pipe. This was particularly the case when the water was allowed to stand in the pipes for any considerable time before being used for drinking purposes. He

mentioned in connexion with this point, a striking instance which had come to his knowledge. It was noticed in a public school that many children suffered from colic after drinking the water which had stood in the pipes since the Friday before. This fact was noticed also whenever an extra holiday had been taken ; but no such symptoms were developed when the water was constantly used. The cause was soon found to be lead poisoning. Faucets were afterwards kept running during all the time that there was no use for the water, and the symptoms immediately ceased thereafter to show themselves. This was only one example of many, proving the bad effects of the use of lead pipe for the conveyance of drinking-water. In order to prevent these bad effects, tin pipe had been substituted, but its great cost interfered with its general introduction. The objection urged in reference to its expense had now been overcome by lining the lead pipe with tin. This compound pipe was much stronger, though lighter, and fully as cheap as the ordinary lead pipe.

In conclusion, he remarked that there could not be much doubt that very many of the obscure nervous diseases which were found to exist among the higher classes were due to the slow lead poisoning induced in the manner referred to. It was fair to suppose that the majority of people received a pretty fair dose with their coffee each morning, as the water which was used in making it was drawn off the first thing in the morning, after it had been standing in the pipes all night. He had frequently detected lead in the water of his laboratory when it has been in the pipes only a single night.

Specimens of the pipe were then exhibited to the Fellows, after which the Academy adjourned.

ADJOURNED MEETING, APRIL 26, 1866. DR. JAMES ANDERSON, PRESIDENT, IN THE CHAIR.

DISCUSSION ON ALCOHOL.

The remarks of Dr. MCCREADY, who, by appointment, opened the discussion, will appear in a future number of the BULLETIN.

DISCUSSION ON CHOLERA RESUMED.

DR. ELISHA HARRIS then gave, by invitation, a report of the inspection of the Steamship Virginia, made by Dr. Dalton and himself for the Board of Health. He stated that the vessel was a large one, was not overcrowded, except so far as very defective ventilation in a portion of the decks was concerned. The first case of cholera which appeared was a very strongly marked and typical one. It occurred in the person of a previously healthy

and strong Hollander when the vessel was six days out from Liverpool. This man died in collapse on the eighth day out. The vessel stopped at Queenstown and took on board some Welsh and Irish emigrants. The march of cholera was very rapid, and in the course of forty-eight hours it had invaded both decks. The disease was of short duration in the fatal cases. The feeblest among the passengers seemed to be those that were attacked. The greatest number were in the worst ventilated localities. The progress of the disease was uniform until the vessel arrived at Quarantine three or four days after—that is to say, a given number would fall sick each week, and a certain proportion of these would die in a given time. The disease was easily controlled during the premonitory stage, and in fact the Surgeon of the vessel stated to Dr. Harris that he had been uniformly successful in checking the diarrhœa. The suffering, according to Dr. Harris's observation of those cases which he saw, was not so severe as in others which he had observed during the last two epidemics, the muscular cramps not being so severe. He had taken special pains to trace the origin of the disease to some passengers that might have come from infected districts. He, however, failed to ascertain anything upon this point, as it seemed from the assertions of the passengers that they had all come from healthy districts. There were, however, one or two suspicious vagabonds who impressed Dr. Harris with the idea that they knew more of cholera than they were willing to communicate. They had been wandering over a good part of Europe, and had at last accounts been in Wurtemburg. After concluding the inspection, he went away with the conviction that the disease might have been introduced by these individuals.

Up to the 18th ult., there had been 250 cases on board; of these 150 had recovered, 50 died, while 50 were still sick. He stated the epidemic had now about ceased both on the Atlantic and Virginia.

In conclusion, he urged upon the Academy the necessity of devising some systematic method of procedure which should be adopted by the city and the towns in the vicinity, in order that we might be prepared properly to meet any emergency that might arise. He then offered the following resolutions, which were adopted:

Resolved, That the Committee on Public Health be further increased by the appointment of three members, and that it be directed to report to the Academy, at its next meeting, upon systematic medical service and preventive measures in epidemic cholera.

Resolved, That the Committee be empowered to call a special meeting according to the provisions of the by-laws in such cases.

Dr. Griscom did not think that there was any very special

reason to apprehend that cholera was coming upon us at once, merely because two ships with the disease on board had made their appearance at this port. The ships, he thought, were the cause of the disease. He then reverted to his theory of the propagation of the disease, viz. that in the first place, a peculiar condition of the atmosphere must exist; and secondly, that it was equally necessary to have combined with this condition local causes in the shape of filth, bad ventilation, etc., etc. He maintained that we could not have one without the other; and that if the proper sanitary regulations on ship-board had been observed the disease could not possibly have broken out. If the tendency to cholera had not existed, instead of having that disease, "ship fever" would probably have broken out. In proof of the correctness of this supposition, he referred to the fact that the disease had not appeared among the cabin passengers. He was, however, in favor of using every precaution against its spread. In conclusion, he urged the practicability, in case that cholera should come, of each and every physician volunteering his services. This volunteer service might be systematized by each physician having a particular locality under his charge; his duties being to hunt from house to house for all patients who had diarrhœa, and to treat them at once.

Dr. Garrish remarked that Dr. Harris, in his account of the sickness on the Virginia, had not alluded to the treatment that had been resorted to.

Dr. Harris answered that the treatment was only efficacious in the early stages, and then capsicum and tinct. opii were mainly administered, while rest was enjoined, and such stimulants employed as could be obtained, either in the shape of brandy, whiskey, or wine. As soon as the cramps set in there seemed to be no chance for recovery by any means of treatment.

Dr. Hubbard asked if the disease was or was not contagious. He could not explain the introduction and spread of the disease in country towns from single individuals coming from an infected locality in any other way than by personal contagion.

Dr. Harris remarked that when the disease made its appearance in any locality, it was sure to spread more or less, and that it seemed to be communicated from one to another; but how this was accomplished he was not prepared to say.

Dr. Henzog, in this connexion, stated that the disease required twenty days for incubation, and that it was possible that one of the suspicious characters referred to by Dr. Harris, on board the Virginia, had brought the disease from Wurtemburg, or Westphalia, where it was reported to have existed during last month. He further stated that all authorities agreed in this, that it was impossible for the disease to find its way into any country if quarantine was sufficiently enforced. That it never could be introduced except by a specific poison carried by an infected person. He did not think that, under the circumstances, there was any

danger that the disease would get to the city from the Virginia; but still, while we were trying to keep it out by quarantine, some of those patients who escaped from the Atlantic at Halifax might bring it by an overland route.

Dr. Bibbins stated that Dr. Clark, in reference to the matter of contagion, had mentioned the important fact that the first case of cholera that had broken out in Massachusetts was in the State Prison, in the person of a prisoner who had been confined there seven years.

Dr. Underhill stated that the last lecture which Dr. J. M. Smith had delivered to the graduating class of 1832, in the old Barclay-street College, was upon cholera. Immediately after graduation in that year, Dr. Underhill had become interested in the study of that disease, and, having at that time nothing else to do, devoted himself to the literature of the subject. The last of June of that year, he was called up in the middle of the night to see two persons who were said to be very sick. On questioning the messengers, he became satisfied that, from the symptoms, the patients had cholera. A few days before, cholera had been reported at Quebec, and a day or two before some deaths from that disease had occurred in the city. The patients were two emigrants from Quebec. He repaired to the house, and found both patients going into collapse. This was about 4 A. M. The parties in the house, hearing that the disease was cholera, refused them help, and, with the aid of some professional friends, the duties of doctor and nurse were respectively performed. They gave injections, rubbed the patients, and administered to all their wants, neither dreaming that the disease was in the least degree contagious. Not a single person in attendance took the disease. As for himself, he could not remember to have been more fatigued, and physically in a more favorable condition to take the disease than at that time. The patients died in the afternoon following, after which the doctor went home, took a warm bath, retired early, and awoke the next morning feeling as fresh as ever.

He was afterwards placed in charge of a Cholera Hospital, and had considerable experience in the treatment of the disease. As far as treatment was concerned, it had only been successful during the first stage.

Dr. Hubbard remarked that Dr. Underhill's recital did not prove that the disease was not contagious, only that the persons in communication with the sick did not take it.

Dr. O. White related his experience in reference to an epidemic which occurred in his native town in Westchester county. On the 14th of July, 1832, he started on a visit to his father, who was a practising physician in that place. He arrived there on Saturday afternoon. On Sunday morning his father was called to see Mrs. Brown, and shortly after a boy was sent by his father for him. The case was supposed by his father to be one of cholera, but never having seen a person afflicted with the disease, he wish-

ed to get the judgment of his son, who had been more fortunate. Dr. W. stated that the case was perfectly well marked. The patient died of collapse that afternoon. The disease spread from this, and, before the epidemic was arrested, one out of every four adults had perished. On investigation, it was found that the disease had been brought into the place by a stage-driver who came up from Harlem, where the disease was then prevailing to an alarming extent.

The Academy then adjourned.

ADJOURNED MEETING, MAY 9, 1866.—DR. JAMES ANDERSON, PRESIDENT, IN THE CHAIR.

DR. GRISCOM read the following paper:

THE MALIGNANT INFLUENCES OF THE UTERUS.

The above nosological designation may appear strange to most professional men, who will look in vain for its definition in the Medical Dictionary, but a description of the cases is relied upon to justify its use. The extensive and potent sympathetic relations of the healthy uterus, and the disturbances of other organs produced by its various pathological conditions, are well known. By the cases now to be described, the powerful influences derived from its simple *mechanical* derangements and displacements, and their complete obviation by the restoration of the organ to its normal status, are clearly demonstrated.

Case 1st.—In the spring of 1861, I was called to see a lady about thirty years of age, several years married, and the mother of a child nine years old, but she had not been pregnant since its birth. She had been a long time in delicate health, and was now troubled with a cough, which she regarded as consumptive. I found her in bed, much prostrated in strength, with considerable nervous excitability, and assured in her own mind that she was in a critical condition. My attention being directed by her fear of phthisis to her chest, a thorough auscultation failed to discover any evidence whatever of pulmonary disease, nor did her pulse indicate any tendency of the kind. Upon careful general examination no symptoms of constitutional disorder of any kind could be discovered, except what pertained to the nervous system, nor was there any cause for this discernible upon general observation. In the absence of any apparent cause for the debility, cough, and other symptoms of deranged health, the thought occurred to me that possibly there might be some derangement of the sexual organs, from sympathy with which other functions were disturbed. After considerable hesitation, derived from the absence of all feeling of local disturbance in that quarter, and disbelief in the possibility of its existence, the patient was prevailed upon to

submit to a digito-vaginal examination. This immediately revealed a complete *retroflexion* of the uterus, the fundus being down nearly on a level with the os, and pressing back upon the rectum, while the neck was thrust forward towards the pubes. Although indisposed to regard this misplacement as the only source of the long-existing disturbance of health, especially of the cough and general prostration, the restoration of the organ to its natural position was a necessity, and was soon effected by the introduction of Simpson's sound, aided by pressure upwards of the fundus by the finger in the vagina. This being accomplished, a horse-shoe pessary was introduced in such a manner that one end occupied the posterior vaginal *cul-de-sac*, the other end resting upon the lower and anterior portion of the vagina, near the pubes, thus aiding to maintain the organ in its normal position, theconfirmation of its restoration being dependent upon time and the avoidance of any jarring motion of the body.

That this simple physical derangement of the uterus was the cause of all the distressing symptoms, including even the cough (which was without expectoration), was made manifest by their entire disappearance from almost the very day when the organ was relieved from its malposition; and that it was the cause of her sterility for so long a time, though the menstrual molimen had been unchecked, was proved by her giving birth to a robust infant within a year from the date of the womb's restoration.

The *Second Case* was an equally striking illustration of the profound and complex sympathetic influences of the uterus, though exerted upon an entirely different class of functions. A lady of twenty-two years, a bride of about four months, who had recently arrived from her paternal home in Ireland, sought my advice in November last for a most distressing attack of dyspepsia. The history of the case revealed the following facts: Before the age of seventeen, she had enjoyed excellent health; at that age her menses commenced and continued regularly for about a year, since which time she has been subject to very irregular and painful menstruation, the function being frequently suspended for from two to four months at a time. Within two or three years, other distressing disorders have also appeared, viz. dyspepsia, constipation, nausea, and vomiting, which occur at frequent intervals, and continue several weeks at a time. The most eminent practitioners of her native country have been consulted for her relief, but, as I was informed, without any beneficial result.

Regarding the case as one of general derangement of the chylopoietic viscera, with special functional disturbances, every remedy indicated by the symptoms was given a fair trial, but the dyspeptic suffering continued unabated. Its protracted severity became so great as at length to lead to the suspicion of sub-acute gastritis, the evidence for which diagnosis appeared sufficiently strong to justify counter-irritation, first by Croton oil, and this failing to mitigate it, then by cantharideal vesication over the epigastrium. No relief followed either of these applications.

Finally, on taking into consideration the coincidence of the amenorrhœa and the constipation, with the dyspepsia, the absence of any distinct cause of either, and the total failure of all the means which, in my experience, had hitherto proved successful in cases apparently similar, the suspicion arose that the uterus itself might be the source of all the trouble. Accordingly, an examination per vaginam was made, and as in the case before related, a complete retroflexion was immediately ascertained to exist. In a few minutes it was by the same means restored to its normal state and so maintained, the recumbent position of body being enjoined for at least a week, in order to avoid all danger of a return of the flexion.

In this, as in the former case, the good result was equally prompt and perfect. Every evidence of gastric derangement immediately disappeared, together with the constipation, and though more than five months have now elapsed since the operation, not one of the symptoms which had rendered the past four years of the patient's life a period of severe and almost continuous suffering, has returned. Daily defecations, punctual and painless menstruation, and a good appetite, without the slightest gastric disturbance, combine to demonstrate how dependent upon each other for their healthy condition are the various parts of the animal structure, and how closely they are correlated by that mysterious influence which has its centre and origin in the brain and spinal cord, and pervades every fibre of the body.

As to the cause of these organic derangements of the uterus, whereby its nervous connexions and associations undergo so great disturbance, and produce these varied sympathetic phenomena, in the first of these two cases, it was impossible to trace the difficulty to any demonstrable circumstance. It probably occurred soon after parturition, several years before, and was due to the non-restoration of the muscular and ligamentous tissues of the organ to their natural contractile tone, whereby it was unable to resist the pressure of the superincumbent abdominal viscera, its own weight serving to continue the flexion, self-rectification being impossible.

In the second case, the cause of the difficulty was more directly traceable to an unusual and severe exertion of the body. About the age of eighteen, the patient was attacked by a horned brute, to escape from which she was compelled to leap a fence. Although no apparent injury resulted therefrom at the time, the irregular menstruation soon after commenced, and was speedily followed by the symptoms of dyspepsia, constipation, and nausea which occurred at frequent intervals until she came under my care. She was otherwise in good health, and in the frequent practice of horseback-riding, which, though a very agreeable exercise, doubtless helped to maintain the uterus in its deranged attitude.

CONGENITAL ABSENCE OF THE UTERUS.

Dr. Chamberlain stated that during the course of his duties as physician to the class of Diseases of Women in the Demilt Dispensary, a female presented herself to him several months ago with icterus and anasarca. Having decided that it was a case of albuminuria, and the patient requiring attention to her physical wants as well as continuous treatment, he sent her to hospital. She reappeared last week with secondary syphilis, which had manifested itself in a general papular syphilide and local mucous tubercles. He then discovered that the *mons veneris* and the *labia* were deficient, and that the *vagina* was nothing more than a *cul-de-sac*, an inch and a quarter in depth. A further examination through the recto-vaginal septum also revealed the fact that the uterus was absent. She was unconscious of her anatomical peculiarity, and although there were repeated attempts at menstruation, there was, according to her statement, no sexual instinct. He would state in addition that the *mammæ* were fairly developed.

HYPODERMIC INJECTIONS OF MORPHIA—TWO CASES.

Dr. Clark, in obedience to a hasty summons, found at the residence of Dr. Budd a lady visitor, afflicted with vomiting, exceedingly restless, nearly pulseless, perspiring somewhat, with cold hands, a livid countenance, and exhausted by copious alvine dejections. In fact, her condition was regarded by both as almost desperate. The first thought suggested was that of poison, which led to an inquiry regarding recent articles of diet. This brought to Dr. B.'s recollection that some six months before, he had prescribed for her the saturated *Tinct. rad. aconit.* in five-drop doses, and suspecting that this preparation might be the origin of her present condition, he questioned her regarding the same. She then stated that despite his injunctions to be very careful not to exceed his directions, she had just after dinner hurriedly swallowed an unknown quantity of the medicine. To bring up the heart's action by alcoholic stimulants was decided upon by both as the plan of treatment to be adopted, and the gastric irritability precluding their administration by the mouth, an injection *per rectum* was had recourse to, but with no other effect than the immediate return of the liquid. Fifteen drops of *Majendie's solution of morphia* were then hypodermically injected, when the pulse, in fifteen minutes, returned, the hands recovered their temperature, the vomiting ceased, and a quiet slumber, which lasted for two hours, was induced. The same symptoms returned upon awakening, but a repetition of the injection produced the same gratifying result, and a third application carried the patient through a refresh-

ing sleep to eight o'clock on the next morning, at which time he found her complaining of nothing but nausea.

The second case was in its bearings somewhat adverse, as revealing a possible accident. A patient who had been the subject of an instrumental labor was received some eight days afterwards into one of our public institutions for the purpose of being treated for a decided trismus, accompanied by a spasmodic action of the limbs. The uterus and vagina were lacerated, the latter forward and a little to the right. The house physician had introduced, by means of the hypodermic syringe, fifteen drops of *Majendie's solution of morphia*, which treatment he himself ordered to be continued, with instructions that the patient be visited every two hours, in order to mark the effect of each injection. In this way four or five injections, at intervals from two to two and a-half hours apart, had been administered in portions varying from ten to twenty drops. At two o'clock A.M., the house physician finding her spasms unrelaxed, her intellect clear, and her nervous system in general still unaffected, again injected twenty drops of the solution. According to the nurse's statement, the patient fell into a slumber as soon as the physician left, but her condition, not being regarded as noteworthy, was not reported. The subsequent visit discovered her profoundly narcotized, from which condition the usual remedial agents, including flagellation, failed to arouse her. She died within an hour and a half afterwards.

At the autopsy, the points which served for the introduction of the injections were carefully dissected, but only about the last one was there discovered an ecchymosis, which led to the inference that the fluid was sent directly into a small vein, and the effect of the drug was thus more promptly as well as decidedly produced than when indirectly imbibed through the medium of the areolar tissue. A small amount of pus in the pelvic cavity was also found, shut in by adhesions following the lacerations, but there was no general peritonitis.

He had been informed by Dr. Hurlburt, of Stamford, Conn., that there had occurred three fatal cases in his neighborhood, which were ascribed to the use of morphine according to the method under consideration. Still he might cite, on the other hand, the case of a physician's sister who had been the subject of three thousand of these applications within the year.

Dr. Batchelder, who favored the exhibition of opium in tetanus, when the urgency of the case so demanded, looked for the production of its effect in from fifteen to thirty minutes; at the end of that time, he thought the dose might be safely repeated. An early and valuable symptom of the approaching subjection of the system to its influence was the passage of the hand towards the nose—in truth, he was guided by it as the very first manifestation of the condition sought to be brought about. In the matter of hypodermic injections, he would advise, as a precautionary mea-

sure, a pinching up of a fold of the skin previous to the insertion of the syringe-point.

A CASE OF PURPURA HEMORRHAGICA.

Dr. Bulkley gave the summary of a case seen by him about a month ago. A lady, aged about twenty-three, was married on a Thursday, and had put up at the Astor House, in this city. Her health remained as good as usual up to Sunday, and then only a certain yellowness of the skin, which was supposed to be due to jaundice, was remarked. On Monday she breakfasted as usual at the hotel table; but a certain redness, which had manifested itself the day previous at the tip of the nose, was beginning to extend; at 2 P. M. of the same day she was seized with nausea, some vomiting and purging. Still she neglected to send for a physician until 10 P. M., at which time he found her condition as follows:—The nose of the color of deep-red wine lees, and each side of the cheek marked by a streak of the same hue; the eyes ecchymosed, the pulse hardly countable, and the extremities cold, together with the presence of some perspiration upon the surface. There was also jactitation and delirium, but coma had at no period of the invasion manifested itself. She had vomited a dark-colored matter resembling blood; she had passed blood from the rectum, and during his attendance, which was continuous, he was informed that there was also a hæmorrhage from the vagina. By the free exhibition of stimulants, and external applications, her pulse became variable, ranging from 118 to 140 and 150, and her skin natural; but she at length rejected both food and stimulants, and died at five o'clock on Thursday morning, just seven hours after he had first seen her. This case he thought remarkable for the rapidity of its termination. His suspicions were at first directed to the possibility of variola being a complicated element, since he had met with several instances of that kind, all of which had terminated fatally; and he therefore sought carefully for evidences of an eruption, but succeeded in finding none. He, however, subsequently learned from the husband that variola had prevailed in the town where she formerly resided, and that she had spent the fortnight preceding her marriage in this city.

Dr. Clark had known cases to prove fatal in a single day, but they had occurred only after the disease had declared itself. He recalled some cases which he had seen in connexion with Dr. B., in which death had taken place within four or five days.

The Academy then adjourned.

Stated Meeting, May 16, 1866. Dr. James Anderson, President, in the Chair.

Dr. Samuel W. Francis, through Dr. Hinton, presented to

the Academy the *Dictionnaire des Sciences Medicale*, in fifty-nine illustrated volumes.

Dr. F. H. Hamilton also donated through the same medium his work on *Military Hygiene*, in two volumes. Both gentlemen received a vote of thanks for their valuable contributions.

RESECTION OF THE ELBOW-JOINT—RESULT.

Dr. Post exhibited in the person of a female aged twenty-one years the result of an operation performed eleven years ago, and which he had undertaken for the purpose of curing a nearly perfect anchylosis, consequent upon a neglected fracture implicating the left elbow-joint. At that time she was unable to bring the hand to the mouth in consequence of the straightened limb. He made use of the longitudinal instead of the transverse incision, in order to preserve the fibrous bands, which contribute so much to the strength of the limb, and removed about one half inch of the deformed end of the *os brachii*. But in his anxiety to save as much bone as possible, he removed too little, and was therefore obliged to resort to a second operation to secure the desired mobility, when about one inch more was removed. On neither occasion were the bones of the forearm disturbed. The arm is not as well developed as its mate, and nearly two inches shorter, but the functions of the joint have been completely restored. She is able to perform her usual household duties, can carry a pail of water, etc.

A CHEILOPLASTIC OPERATION.

Dr. Buck illustrated by two plaster casts and several photographs the original deformity resulting from the healing-up of a shell wound, and its progressive mitigation under surgical treatment, the particulars of which had been previously given to the Academy.

Dr. Stephen Smith, from the Committee on Public Health, read the following

REPORT

ON THE

MEASURES OF PREVENTION AND RELIEF

TO BE ADOPTED DURING THE PREVALENCE OF

EPIDEMIC CHOLERA.

By STEPHEN SMITH, M.D.

An eminent writer has observed that "cholera should not be treated as a disease, but as a pestilence." The truth of this remark becomes more and more apparent with every succeeding epidemic. All our efforts to control it by the well appointed agencies which are applied efficaciously against ordinary diseases have proved for the most part unavailing. To control its progress, to mitigate its severity, and to avert its attacks, we must resort to special measures, and apply them with promptness and precision.

Your Committee has been directed to report upon those measures of prevention and relief which it is necessary that municipal authorities and Boards of Health should adopt during the actual presence of cholera, in addition to the prosecution of necessary sanitary works.

In order to a proper appreciation of this subject, we have to inquire, 1st, What are the lessons which the past epidemic visitations have taught us? and 2d, How are these lessons to be applied?

1. Cholera epidemics have taught us that cholera is almost invariably preceded by a diarrhœa. This diarrhœa is generally painless in its character, and precedes the stage of collapse for a variable period, often continuing for days.

Diarrhœal diseases also prevail over large tracts of country, preceding or accompanying cholera. In 1832 an enormous amount of diarrhœa occurred in the towns of Europe which were visited with cholera. In 1849, in all the towns of England attacked by cholera, diarrhœa prevailed to a remarkable extent; in Glasgow, during the height of the epidemic, nearly the whole population was affected. The same remark applies to the towns of this country visited by the pestilence.

2. They have taught us that the special importance to be attached to this diarrhœa, during the prevalence of the epidemic,

is its liability to terminate in fully developed cholera; in other words, that it is the premonitory stage of cholera. The English Board of Health state as the unanimous conclusion of large numbers of medical men in 1849, that "whenever diarrhœa prevails extensively in a country and district where cholera is epidemic, diarrhœa is premonitory of cholera; that it is not a mere coincident or concomitant; that it is not even merely a predisposing condition, like a multitude of other circumstances, but is part and parcel of the disease, not to be distinguished from the actual commencement of the most severe form of the malady."

3. They have taught us that this painless diarrhœa rarely excites apprehension. This is known to be true, even where the victim is a well-informed physician. Mr. Aston Key, then one of the most brilliant surgeons of London, lost his life in 1849 by persistent neglect of the premonitory stage. Dr. Cooper, long Health Officer of Southampton, England, during the slight epidemic of last fall in that town, was constantly urging the people to promptly attend to attacks of diarrhœa; and yet, at a dinner party, he informed a medical friend that he had suffered from a mild diarrhœa for several days. On being remonstrated with for his neglect of himself, he declined treatment, and the diarrhœa soon passed into fully developed cholera, and he died. If even physicians are thus indifferent, what must be the apathy of the laboring classes? All observers agree that it amounts to the most reckless carelessness. Mr. Grainger, Inspector of the General Board of Health, remarks of the poor of London: "To those unacquainted with the actual facts of the case, the extent to which the poor, during the epidemic, neglected the premonitory diarrhœa, must appear almost incredible; notwithstanding the measures adopted by the authorities, to advertise them of the necessity of early application, they were generally, when first seen by the medical officers, already in collapse; their ignorance of the connexion between looseness of the bowels and cholera, the apparently slight nature of the attack, and especially the absence of pain, lulled thousands into a fatal apathy and security."

Dr. Sutherland, also an Inspector of the General Board of Health in 1849, and who consequently had a large field of observation, reports: "A very ample experience has convinced me that those who are in the most danger are least likely to apply, because there is a state of the nervous system connected with a severe epidemic seizure, the tendency of which is to make the sufferer apathetic. The sentient nerves are dulled, and important constitutional changes take place without pain. The discharges which are sapping the very powers of life are permitted to go on, not only without check, but with a certain consent to the feelings of relief which are experienced. No alarm is taken till it is too late; and, in not a few instances, the relatives have been first aroused to a sense of danger by the last death struggle of the patient; it

has likewise happened that the medical visitor, in going his rounds to seek out cases of diarrhœa, has found the dead bodies of those for whom no medical aid has been sought or procured. Fifty-one such examples occurred in one parish in Glasgow alone." He adds: "It has been found by melancholy experience, both in Dumfries and Glasgow, that neither rich nor poor will, of their own accord, apply for medical aid, until the time for its effectual exercise is either past, or the chances of recovery reduced to a very small proportion. The premonitory disease, in a large number of persons, is attended with sensations rather agreeable than otherwise, the sufferer is lulled into a fatal security, and no alarm is taken till it is too late."

4. We have learned that the premonitory diarrhœa is very curable. Statistical inquiries show, that not far from 99 per cent. of the cases of diarrhœa, occurring during a cholera epidemic, recover if properly treated. Of 44,737 cases of diarrhœa treated in several parishes in England, where cholera was raging in 1849, but 52 passed into cholera. In several other districts 26,803 cases of diarrhœa were treated, many of which had advanced to the stage of rice-water purging, but 11 terminated in cholera.

5. Finally, we have learned that cholera attacks given insalubrious localities, and does not spread generally over a town. Nor does it attack these centres of filth and overcrowding simultaneously, but rather in succession. After a period, varying from ten to twenty days, the epidemic tendency generally subsides, and the locality may or may not remain free from subsequent attacks.

The following propositions may then be regarded as well established:

1. That epidemic cholera is almost invariably preceded by a diarrhœa.

2. That this diarrhœa is identical with cholera in its progressive stages.

3. That little or no apprehension is excited by the premonitory diarrhœa, and hence that it is allowed to pass untreated, until it terminates in developed cholera.

4. That this diarrhœa is, in the vast majority of cases, curable.

5. That cholera attacks the most insalubrious localities in succession, and expends its force apparently within two or three weeks.

Taking these facts as our guide, it is not difficult to determine what measures of prevention and relief should be adopted by those bodies having in charge the health of communities during the actual presence of Asiatic cholera, in addition to the prosecution of the proper sanitary works.

In the first place, it is evident that the ordinary administration of gratuitous medical relief, through the medium of the Dispensaries, will not meet the emergency. The agencies of the Dispensaries reach only those who apply for relief, but the victims of cholera do not, as a general thing, make application for relief.

Any system, to be effectual, must be aggressive, not passive. It must send its agents to the homes of the people, and by the most systematic and persistent effort search out the pestilence in its incipient stages, and apply the remedy at the bedside. It must be a house-to-house visitation, so thorough as to actually reach every member of the family. We are not left to conjecture that this will prove an effectual preventive measure during the prevalence of cholera. Examples of its adoption and successful prosecution are on record, and deserve our careful consideration. They furnish indubitable proof that if the system of person-to-person or house-to-house visitation be vigorously prosecuted, and all the premonitory cases be found out and treated, the epidemic, however severely it may be raging, is positively under the control of the local authorities.

II.—HOUSE-TO-HOUSE VISITATION.

The great importance of treating the premonitory stage was fully recognised by the Special Medical Council of the Board of Health of New York, as early as 1832, as will be seen by the following quotation from Aitken's history of the epidemic of that year. The Board of Health propounded to the Special Medical Committee, the Chairman of which was Dr. Alexander H. Stevens, the following question:

"If, during the premonitory or warning stage of malignant cholera, a physician acquainted with its action upon the human frame be called to the patient, what proportion of a given number of persons of ordinary constitutions, can, by known and certain medical means, be secured against death by the threatened attack?"

To which the Committee returned answer: "If proper medical advice were taken, and judicious application of medicine made upon the first slight appearances of disorder of the stomach and bowels, ninety-nine persons in a hundred, of temperate habits and ordinarily good constitutions, would be safe from the attack of malignant cholera. That is to say, the number of deaths from cholera, if the disease were uniformly prescribed for in the stage of diarrhœa, would be exceedingly small."

It is well worthy of notice, also, that the plan of searching out the premonitory cases and promptly treating them was adopted at Bellevue Hospital, then under the superintendency of one of the most judicious practitioners of the city, Dr. Isaac Wood, and with the most happy results. So promptly was the epidemic suppressed in that institution by this measure, that the circumstance attracted the attention of the Board of Health, and a committee was appointed to report the facts in the case. The Committee state, "That the medical officers of the Bellevue establishment, in all its departments, have taken the most efficacious measures, as

well as most fortunate in their results, for enforcing among all the persons under their charge an undeviating attention to the recommendation so earnestly enjoined by the Special Medical Council, of making known, at the first onset, the forewarning symptoms of cholera, and of obtaining immediate medical assistance. * * * No longer confiding to the inmates of the Bellevue establishment the care of reporting their own illness, the medical officers themselves go round daily, and question every person individually, and whenever diarrhœa is detected, medical treatment is ordered on the spot. From twenty to thirty such cases are prescribed for each day, and it is the impression of the officers that many of those cases, if neglected, would still pass into cholera. The same medical police is observed on Blackwell's Island; and the Penitentiary prisoners residing there are daily paraded, questioned, examined, and inspected, and if any are found to present the premonitory symptoms, they receive instant medical treatment. * * * Coincident with the strict enforcement of this system, the disease has been seen to decline, and at last to cease. * * * The entire cessation of the disease at Bellevue, without the cause having exhausted itself, in consequence of those measures only of precaution and care which in like manner and in other places and in other hands have been so successful when applied in time, is highly encouraging."

The plan of searching out the premonitory cases was carried out on a very limited scale in this city, in 1849, but with very satisfactory results. The Medical Committee, consisting of Drs. Beck, J. M. Smith, and Moore, state in their report: "The parts of the city where the cholera prevailed was divided into districts, and physicians were appointed to each district for the purpose of visiting the houses of the poor, with the view of ascertaining the existence of disease and affording immediately the necessary relief. The number of physicians appointed to the discharge of this duty was twelve.

"The amount of good accomplished by these measures, especially the last, in the way of preventing the development of disease, the Committee believe to have been great."

In England, attention seems to have first been directed to the plan of arresting epidemic cholera by treating the premonitory diarrhœa of those exposed, by the success which attended the opening of a dispensary at Bilston, by a Dr. McCann, in 1832, to which the poor were induced to apply for relief in the early stages of the disease. The dispensary was opened when the epidemic was raging most severely; but within five days the cholera death-rate was greatly diminished, though the diarrhœa continued unabated, and in twenty days the fatal cases ceased altogether. The report states that "during the first five days from the opening of the dispensary, the number of fatal cases, as compared with the number in the preceding five days, had decreased from 227 to 134; in the second five days the number was reduced to 59; and

on the eighth day from the expiration of that time, being the 18th from the opening of the dispensary, not a single fatal case was reported."

On the approach of the epidemic of 1848–9, the General Board of Health of England, influenced by the success of this example, reduced the whole procedure to a system. This was the first effort in Europe to arrest the progress of cholera by the systematic treatment of its premonitory stages.

In giving their reasons for its adoption, the Board of Health remark: "Satisfied that the connexion between diarrhœa and cholera, though so certain and of such vital practical importance, would not be understood by unprofessional persons, and especially by the poorer classes, we were apprehensive that a great loss of life would be the consequence of leaving the poor to themselves until they should of their own accord apply for relief. It appeared to us that it was not sufficient merely to appoint an additional staff of medical officers to be ready to give their assistance when summoned, but that it was necessary to send those officers, provided with the appropriate remedies, into the infected localities, and even to the very houses of the poor, to examine the inhabitants in their own homes and while engaged in their ordinary occupations, and in this manner to commence the treatment of the disease wherever it should be found to exist, before the persons affected were themselves conscious that they were the subjects of it. This seemed to be the only effectual mode of dealing with a pestilence the peculiar character of which is that it runs its moral course in a few hours, and passes wholly beyond the control of human aid and skill unless preventive measures are taken against it in its very earliest stage." "The practical trial of the system of house-to-house visitation," they add, "brought out the evidence of the ignorance and neglect of their perilous condition on the part of all classes, but particularly of the poor, to a larger extent than could have been anticipated."

The objects aimed at by this preventive measure are thus summed up by Dr. Sutherland, of Glasgow, who perfected the system and carried it into full effect in that city in 1849. They are:

"1. To prevent persons who might not apply for medical aid, even in cholera, from dying without such aid.

"2. To seek out neglected cases of cholera, so as to bring them under treatment at the earliest possible period, and thus diminish the mortality of the epidemic.

"3. The discovery and immediate treatment of every case of diarrhœa in localities where cholera prevailed, and where the patients had not applied at the dispensaries, in order to prevent, as far as possible, the development of the disease.

"4. To keep a constant medical inspection over affected districts and houses, to insure their being preserved in a proper sanitary condition.

5. To exercise a moral agency over the population by giving such instructions in regard to cleanliness, ventilation, and personal habits as might appear needful, and by explaining and enforcing the necessity for immediate application to the dispensaries or medical officers by all parties who might be taken ill during the intervals between the daily visits."

The following facts illustrate the value of this method of relief:

Dumfries, with a population of 10,000, was the first town in which the experiment was tried. The epidemic seizure was a single one; and all the cases, almost without exception, were preceded by neglected diarrhœa. Before arrangements were made 147 people had perished, and before the visitation was in full operation 250 deaths had occurred. During the three first days, in which it was only partially in use, the fresh attacks daily were respectively 37, 38, 23, and the deaths 9, 6, 9. On the three succeeding days, when it was in full activity, the attacks diminished to 11, 14, 12, and the deaths to 7, 3, 6. On the following three days the attacks sank to 8, 4, 2, and the deaths to 6, 4, 5. In three days more the epidemic was at an end, a few isolated cases only occurring during the succeeding eight or ten days.

Charleston, a suburb of Paisley, was placed under active visitation at a period when the new attacks of cholera amounted to 23 daily. On the fourth day after the system was in full operation the attacks fell from 23 to 3 daily, and in a few days more the pestilence ceased.

In Inverness 20 cases of choleraic diarrhœa occurred, 10 of which, being neglected, proved fatal; the other ten, being brought under visitation and treatment, all recovered.

In Glasgow 13,039 cases of premonitory diarrhœa were brought under visitation, 1000 of which had passed to the stage of rice-water purging, yet of the total number but 27 passed into cholera.

In the Shoreditch district, London, there were brought under treatment 21,116 cases of premonitory diarrhœa, 342 cases approaching cholera, and 197 cases of developed cholera; of the number of cases of diarrhœa discovered on visitation only one, and of the cases approaching cholera only two, are known to have passed into cholera.

The conclusion of the Board of Health at the close of the epidemic, in regard to the general and uniform results of the adoption of this system, are thus stated:

"1. The discovery of a number of dead bodies—the individuals having died of cholera without having received any medical assistance whatever.

2. The discovery of a number of cases of cholera in various stages of development, proceeding with different degrees of rapidity to a fatal termination, not only without medical assistance, but without the slightest apprehension of any danger on the part of either the sufferers or their friends.

3. The discovery of a vast number of cases of diarrhœa, some of them bilious, some with rice-water purging, and others advanced to the stage characterized by serous discharges, without any medicine having been taken, without any alteration having been made in diet, without any thought of sickness, and much less any apprehension of the commencement of a morbid ailment.

4. The application of a great number of persons to the various dispensaries for the immediate and gratuitous supply of medicines, the opening of which in convenient situations formed an essential part of the visitation system.

5. A gradual and progressive diminution of the developed, and an apparent increase of the premonitory cases, the diarrhœa taking the place of cholera.

6. A decided diminution in the number of attacks.

7. A decided diminution in the mortality.

8. Sometimes a rapid cessation of the disease, and invariably a decided and steady progress towards it."

Such are some of the proofs of the value of home visitation when systematically carried out. Many more might be added, but these willsuffice.

When the plan was first proposed in England it was received with great scepticism by the community, and even by the medical profession. It was believed that the organization would prove impracticable; but the Board of Health state that "on the actual trial of the system, it proved to be far more easily carried into effect than was at first apprehended. The presumed difficulty of obtaining an adequate number of qualified persons to undertake a work apparently so extensive and dangerous, was never a practical one. Experience has shown that in the most violent and extensive outbreaks of the pestilence, its virulence is invariably confined to circumscribed localities. Even in the districts the most severely attacked, the great bulk of the mortality always occurs within a very limited space, while the disease seldom lasts long at any one point, but attacks a number of points in succession. This is the law of the epidemic. A large staff of visitors, therefore, is not required. A small number, properly organized and directed, are capable of commanding a very extensive district, and may hunt out the disease therein wherever it exists; but this service requires to be performed with all the precision of a military movement." It was also alleged that this system would meet with opposition or indifference from the people, and thus prove impracticable. But the universal testimony of the visitors was to the effect that the poor welcomed them and were very grateful for their services. One says: "The poor almost universally regard the system of house-to-house visitation as a great boon." Another writes: "They have looked upon our staff as messengers of mercy, and welcomed us with many kind outpourings of grateful hearts; they felt that, though hitherto uncared for in their dire and dreadful calamity, at last the Board of Health has come

forward as beneficent guardians of their health, and quieted alarm by efficient preventive measures." Another says: "The medical house-visitation was received with the greatest thankfulness. In the overcrowded districts the poor eagerly told the visitors their complaints, and received the medicines most gratefully. The instructions and advice were eagerly obeyed, and the visits were looked for in future."

The organization of the system of house-to-house visitation is very simple. The agencies selected may be lay or medical. The latter, however, are to be preferred, as they naturally readily become familiar with all the details of the work, and prosecute it with more thoroughness. There is first selected a Superintendent; second, such number of assistants as he requires; and third, a corps of visitors. The town is then divided into convenient districts, and an assistant with a proper number of visitors is assigned to each. When cholera appears in any district, or an unusual increase of diarrhœal diseases, the visitors commence their visits. Each carries some simple remedies, and wherever he finds diarrhœa he either treats it, or directs the patients to their physician, and insists upon immediate attention, or, if there is a dispensary, sends them to it. At all events, the diarrhœa is at once put under efficient treatment.

In this connexion we deem it important to notice the plan of house visitation adopted by the New York Metropolitan Board of Health, preparatory to the apprehended epidemic of cholera in this district. The basis of the organization is the dispensaries of metropolitan sanitary districts. These dispensaries have long been recognised by the laboring classes as centres of medical relief, and hence during an epidemic visitation of cholera become important auxiliaries to any system of gratuitous aid. They are generally under a rigid system of discipline, and the physicians attending them are the best of the junior portion of the profession. But what is of most importance, they have their districts accurately defined with district medical visitors, many of whom have been physicians to the district for so many years as to have become perfectly familiar with the habits of the people, the locality of special epidemic diseases, etc. And, finally, these dispensary districts in the aggregate cover New York and Brooklyn.

Another very important advantage which the dispensaries give is the daily record of the prevailing diarrhœal diseases of the district. This enables the resident physician to determine in advance of the appearance of cholera where its foci are to be, and to institute prompt measures of suppression. And if all the dispensaries made daily reports to the Board of Health of the diarrhœal diseases of the day before, there would constantly be at the Central Bureau that degree of information of the epidemic tendency to diarrhœa in all parts of the sanitary district, that would daily indicate the movements of the epidemic, and enable the proper officer to anticipate attacks, and hence ward them off.

There was furnished, therefore, at hand, the groundwork of a most excellent organization for the systematic and complete administration of medical relief. The authorities of the various dispensaries readily assented to the proposition of the Board of Health to coöperate, and the following plan was adopted:

"1. The several chartered dispensaries of the Metropolitan Sanitary District shall be constituted under the Board of Health, District Centres of Medical Relief.

2. Each Dispensary District shall be divided, from time to time, into as many sub-districts as, in the discretion of the Director of the Medical Relief of the Board of Health, are necessary to the efficient administration of gratuitous medical relief.

3. The following officers shall be appointed by the Board of Health, subject to the approval of the Trustees of the dispensary, namely: 1. A District Director of Medical Relief, who shall have the position of Resident Physician of the dispensary. 2. A District Physician to each of the sub-districts of the dispensary.

4. The officers hereby appointed shall be under the exclusive control of the Board of Health only so far as their duties relate to the special measures of medical relief adopted by said Board.

5. The Board of Health engages to supply the District Physicians with such remedies for immediate administration as the Director of Medical Relief may select."

Proceeding on this basis, the organization of the cities of New York and Brooklyn has been perfected. The House Physicians of the dispensaries have assumed their duties, and divided their districts into an average of ten sub-districts, and located a physician in each. New York is thus divided into sixty districts. The organization stands thus in New York:

1. Six Dispensary Districts.
2. Six District Centres of Medical Relief.
3. Six District Directors.
4. Sixty District Physicians.

Although this local force, so judiciously distributed, would seem to be ample for any ordinary epidemic, yet it has been found that the real source of failure is the want of visitors for emergencies. To give, therefore, the greatest possible efficiency to the work, a reserve corps of physicians has been organized, which can be detailed in such numbers as may be necessary to meet any given exigency.

The working of the plan will be as follows:—"The Resident Physician of the Dispensary will furnish daily reports to the Board of Health of all the diarrhœal diseases of the district, thus enabling the Central Bureau to have cognizance of the epidemic tendency in all parts of the Sanitary District covered by dispensaries. Whenever a case of cholera is reported at a dispensary, the resident physician will immediately direct the physician of the district to visit the house and commence a family visitation,

searching out all diarrhœa, and placing it under immediate treatment. If it shall happen that there is more diarrhœa than a single physician can properly attend to, the resident physician will call for more, and such a number from the reserve corps will be detailed to aid in the visitation as will effectually place the cholera field under rigid surveillance until the epidemic tendency has subsided.

This organization is now complete and ready for effective work. Such are the elements which compose it, and so thoroughly are the agencies distributed over the field, that there is every reason to believe that the epidemic may, in a great measure, be controlled.

II.—HOUSES OF REFUGE.

Auxiliary to the work of house-to-house visitation, is the opening of suitable houses for the temporary accommodation of those families which are removed from their apartments for the purposes of cleansing, or because they are unfit for further occupation. In this city the necessity for these establishments during an epidemic of cholera is absolute. Hundreds of families will be found living under such circumstances that, if they are left in their foul apartments, the mortality would be frightful. They must be removed until their dwellings are placed in a good hygienic condition, and then restored to them.

This plan was put in operation in Edinburgh in 1832, with the most satisfactory results. In 1849, it was adopted in many English towns with equally good results. Dr. Sutherland, the agent of the English Board of Health, states that in the severe outbreaks of the pestilence witnessed by him in provincial towns, 87 per cent. of attacks took place in houses where more than one person had suffered from the disease; and that when the attacks thus occurred in groups in the same or adjoining houses, the danger to the inhabitants was enormously increased by leaving them in their dwellings; while very few attacks, and scarcely any deaths, took place among these people, if they were removed from the infected locality to houses prepared for their reception, and were kept there in comfort and under observation until their own localities and rooms were cleansed.

An instance strikingly illustrative of the value of Refuges occurred at Bristol, England. During the epidemic of 1832 cholera appeared in a certain lodging-house, and 35 persons suffered, of whom 33 died. In 1849 cholera again attacked the occupants of this house, but they were immediately removed, 64 in all, 49 of whom went to the House of Refuge. Out of the entire number removed not a case of cholera occurred, though many suffered from diarrhœa, which was immediately put under treatment. Though the inmates of the House of Refuge were taken from the very foci of cholera, yet the mortality was

invariably slight. Out of 270 inmates of the refuge opened in Edinburgh, no cases of cholera occurred. Out of 807 inmates of two refuges in Glasgow, 25 cases occurred, with but 8 deaths; in Dundee, out of 250 inmates there were 4 attacks, and no death; in Sheffield, out of 145 inmates there were 4 attacks and 2 deaths; in Bristol there were 210 inmates and no case. The total number of inmates in all the refuges was 1,691, with 33 attacks and 10 deaths.

Mr. Grainger, who superintended the operations of the Board of Health in London, reports: "It is one of the best established facts that in the management of cholera there is not, next to prompt medical aid, any measure susceptible of immediate application so effectual as the removal of those who are yet well, but threatened with the pestilence, out of the crowded and miserable abodes usually selected for attacks of the epidemic. * * * The testimony of the medical officers was uniform as to the enormous evils that resulted from the impossibility they experienced of removing families living in single rooms where one or more of their members were attacked. In every part of the metropolis these instances were constantly recurring; members of the same family were again and again attacked in succession; as many as three, four, five, and six persons succumbing, one after another, in the same house.

In several instances the poor quarters of towns were depopulated, by removing all who would consent to removal to high, open grounds, where tents or barracks were prepared for them. The result was that those removed escaped with only diarrhœa, while those who remained, with few exceptions, perished. It was a noticeable fact also that when families were removed to houses of refuge, and had their apartments cleansed, and were themselves bathed and fed, they remained well after their return to their old quarters.

III.—CHOLERA HOSPITALS.

A third measure of relief, hitherto much relied on, is the hospital. In every epidemic of cholera, hospitals have been early established, and great importance has been attached to them. But it is questionable if in this particular we are not again treating cholera as an ordinary disease, and losing sight of the fact that it is a pestilence governed by peculiar laws in the progressive development of its several stages. Whoever has had charge of a hospital which received cholera patients must have been struck with the fact that large numbers of them on admission were in partial or complete collapse, although when they left home they were reported to be in the earlier stages. A consultation of the records of cholera hospitals in this and other cities confirms the opinion that many cases pass into the state of collapse while being transferred to a hospital.

Dr. Sutherland says: "I have known a patient taken out of bed with a warm skin and a good pulse, arrive in a state of fatal collapse at the hospital, though not above a quarter of a mile distant." Dr. Adams, of Glasgow, remarks: "At first, when I had all my experience to gain with regard to the treatment of cholera, I was favorably disposed to the employment of hospitals, and looked with painful apprehension to the treatment available to the sick poor residing in dwellings abounding in negations, *sans* good fire, bedding, clothing, light, air, quiet, attendance, etc. I am now, however, clearly satisfied that a pauper patient lying on his wisp of straw, on the bare floor, with a relation or other attendant to supply him with a drink of cold water and to surround him with a few hot bricks, has the chance of recovering fearfully diminished by removing him to all the comforts and refined treatment of a hospital. If my expérience on the subject were singular, I would hesitate to venture so decided an opinion; but from careful inquiry which I have made among many of the parochial surgeons, I find their experience so entirely corroborative, that I feel no hesitation in condemning the principle of hospital treatment for cholera patients."

Hospital treatment, as compared with home treatment of similar cases, shows largely in favor of the latter. By careful estimation it appears that in Liverpool and Glasgow 53.8 per cent. of the cases admitted to hospital died, while in the home treatment but about 36 per cent. died.

The English Board of Health make the following statement: "From the experience of Great Britain and other countries in 1831–2, we came to the conclusion that the treatment of cholera patients in hospitals was not successful, and we discountenance the use of these establishments, recommending that the best provision practicable should be made for affording assistance to the individuals who might need it at their own homes, particularly by the selection of proper persons instructed as nurses in the special services required on the occasion, and paid for devoting their whole time to attendance on the sick at their own habitations, under the direction of medical officers."

The evidence would seem therefrom to be conclusive against hospitals as formerly established for the treatment of cases of cholera. It cannot be denied, however, that cases will occur in large towns where the patients will be so destitute that they must be removed from their domiciles for treatment. For such persons provision must be made, and the form of a hospital must be maintained. But such hospitals should be very small, and in the immediate neighborhood of the destitute class. It should be borne in mind that in the transportation of cholera patients, distance is a most important element. Of the cholera hospitals in Liverpool in 1849, those situated in the infected district had a mortality of 7.6 per cent. less than those situated at a distance from the people who entered them. At Glasgow the mortality

was 10 per cent. in favor of the hospitals near the cholera foci. Whatever hospitals are established, therefore, for the treatment of cholera patients, should be located in the immediate vicinity of those for whose benefit they are designed.

CONCLUSIONS.

Your Committee conclude, in consideration of the preceding facts, that municipal authorities and Boards of Health should adopt as special measures of prevention and relief during the actual presence of epidemic cholera:

1st. A system of gratuitous administration of medical relief at the houses of the laboring classes, so regulated as to reach all cases of premonitory diarrhœa, and united with instruction and advice.

2d. Refuges should be opened at convenient and healthy localities, for the temporary residence and purification of those whose domiciles require cleansing and disinfection, or to be permanently closed.

3d. Hospitals for the reception of patients suffering from cholera should be very small, and located in the immediate vicinity of the cholera focus.

The Academy then adjourned.

SPECIAL MEETING, MAY 30TH, 1866. DR. JAMES ANDERSON, PRESIDENT, IN THE CHAIR.

DR. SQUIBB, as Chairman of a sub-committee under the Committee upon Public Health, submitted the following:

REPORT ON DISINFECTANTS.

WITH SPECIAL REFERENCE TO THE PREVENTION OF

EPIDEMIC CHOLERA.

By E. R. SQUIBB, M.D.

To Dr. John C. Draper, *Chairman*:—Conforming to your request, I beg to submit my views in regard to the best and cheapest disinfectants adapted to general use.

It is not the object of this report to embrace the principles involved in disinfection, nor a consideration of all the important disinfectants; but merely to epitomize, in a practical form, the knowledge of a very few of them; knowledge which is easily and equally accessible to all mankind, though possessed by your reporter to a limited extent only.

For some generalizations upon the principles involved in disinfection, and some account of a few disinfectants not to be alluded to here, as well as for some other matter in relation to this subject and not reproduced in this report, your reporter begs to refer to an article written but a short time since, and to be found in the columns of the *Medical Record* of New York, Vol. I., Nos. 5 and 6, for May 1 and 15, 1866. The first part of that paper was already published, and the second part in type, when your reporter was notified of his appointment to your committee, and the duty assigned to him of presenting this report.

It can never be wholly useless to reiterate and reaffirm on every proper occasion the most important truth connected with this whole subject, namely: that cleanliness and ventilation are the only true disinfectants; and that just in proportion as these are attained, all other disinfectants lose their importance.

But leaving these, the only true disinfectants, out of the question, we still have the common acceptation and scope of the term left; and disinfectants in their common acceptation, and that which is to be their meaning in this report, may be defined to be slovenly substitutes for cleanliness and ventilation, which possess the power in various degrees of rendering filth or uncleanliness less noxious. These substitutes must vary in character with the forms and conditions in which the uncleanliness presents itself, and hence it is probable that no two agencies are equally applicable to the same use. It is, therefore, the proper

selection and application of a known means to a given end, rather than the finding out of new means, which forms the proper study in connexion with this subject.

There is, however, one general agency, which in efficacy, universality, accessibility, versatility of application, and harmlessness, may be regarded as the greatest disinfectant known, namely, heat. The new light and force given to the views of Count Rumford and Sir Humphry Davy, by the researches of the present time, upon the nature of heat and motion, render it probable that heat is the only agency that is disinfectant at all. Or, in other words, that abstract cleanliness once departed from, is only recovered through the agency of heat. This view would explain how it happens that active oxidation or deoxidation is equally disinfectant. All reactions, physical, chemical, and vital, are the result of, or result in heat or motion; and as these are now shown to be convertible terms—or rather two expressions of one and the same agency, therefore every action in which this agency is involved, namely, the operation of every natural law of the universe upon every other natural law, bears through it an intrinsic reciprocal relation. The wide scope of this field, or this train of thought, however, is not adapted to the intended character of this report, and hence heat as a disinfectant was entirely omitted from the first draft of this report submitted to your committee, though not from want of a due recognition of its importance. This brief and inadequate allusion is introduced at the suggestion of your committee, and may be concluded by simply adding that the empirical use of heat as a disinfectant was discovered and applied very long ago. How long, it seems now impossible to tell. The first application of heat in this direction met with, in the reading of your reporter, that can be now recalled, was that by M. Appert, translated into English in 1812 ("The Art of Preserving all kinds of Animal and Vegetable Substances for several Years"). The more special use of heat as a sanitary disinfectant, seems to have originally grown out of its use long ago for destroying the insects and eggs in various insect plagues, and particularly in lousy clothing. It appears to have been known as destructive of the itch contagion by clothing before the acarus was demonstrated in 1812, and it has gradually and empirically grown up to its present rational application, where its claims have been so often and so ably illustrated among us that it can gain nothing at the hands of your reporter.

The cold of a freezing temperature renders the processes involved in the causation of zymotic and epidemic diseases latent and harmless, and a thorough disinfection appears to have resulted from the use of ice. Yet it is considered doubtful whether the effect of cold ever does more than suspend the activity of the causes of infection. The effect of heat, however, is unequivocal. A temperature of 150° is said to be sufficient to destroy the in-

sects and ova of the pediculus and acarus affections (Aitken), whilst 200° is required for the destruction of scarlatina poison (Henry). As, however, few of the articles of common usage are injured by a boiling temperature (212°), this, whether applied dry or moist, seems to be practically adopted as the lower limit of security and safety. Both dry and moist heat have their special and equally important applications, and any advocacy of either to replace the other in general applicability, partakes of the irrational character of too many of the theoretical abstractions of the present day. The use of scalding water or steam, as an application of heat to soiled clothing, which may be cleaned but not materially damaged thereby, is too plainly indicated to admit of question; and the same may be said of the application of dry heat to bales of dry-goods, and most other merchandise that is capable of propagating disease. But whether any such treatment is effective beyond the immediate scope of its application—important though this be—appears doubtful.

All that your reporter's reading has taught him (for from practice he knows nothing), tends to the belief that infection, whether through epidemic, endemic, or contagious influences, starts and spreads, through a chemico-vital process not well understood, called fermentation. That this process requires a special seed, and then a particular climate and soil, and these in a very definite concurrence, to start infection at all. And then, enough seed being present, the degree of this concurrence between climate and soil becomes the measure of the infection which results. Secondly, that the seeds of the various zymotic diseases, though never altogether absent from their natural haunts, are often more widely disseminated, and may be carried anywhere, with or without effect, in proportion as the due concurrence of climate and soil is or is not found. Thirdly, that the natural or artificial climate favorable to some of these diseases, occurs with some regularity during the revolution of the seasons of every year. And fourthly, far most important of all, that the soil, or pabulum vitæ, has also its periodicity, and often, if not always, approaches insidiously to pestilential activity by the way of uncleanliness and bad ventilation, fructifying the seed of an exotic or an indigenous disease, by delicate differences in its nature and character, or different concurrence with the climate.

The word "seed" used here for familiar illustration, in its common connexion with climate and soil, is not appropriate, but must be substituted by the word spore, which, though originally of the same meaning, is now applied to a stage of development much earlier in the progress of life. A seed properly defined, is that stage of advanced cell development where an embryo is recognisable, and a halt made in the progress of development begun long before; and the term is restricted to the matured germ for the reproduction of phænogamous or flowering plants. The term spore is applied to the reproductive germ of crypto-

gamous or flowerless plants, and instead of being an orderly organized aggregation of cells into a definite type or embryo, it is the original homogeneous body from which, upon finding the proper pabulum, the first cell is developed by imbibition from that pabulum. The cell then becomes the second step in the order of vital progression, and in some of the lower orders is the perfect being, reproducing other cells or beings by budding, and without reverting back to the spore so long as a supply of pabulum is kept up. The pabulum being exhausted, however, a spore and not a cell is produced, to be wafted about till it meets the proper pabulum in the condition for fructification, or is caught and decomposed in some of the slow combustions of nature. It next appears that these spores are not albuminous, but become so only in the second stage, or as cells; and hence, as spores, are not, like seed and cells, liable to coagulation and destruction by that low degree of heat which changes albuminous and starchy matters. Payen's experiments appear conclusive upon this point, since he subjected the spores of the orange-colored bread-mould of Paris to a moist temperature of 248° without injury; but at 284° decomposition occurred and their vitality was destroyed. Dr. D. Child (Proceedings of the Royal Society of Great Britain for 1865) showed that living bacteriums were produced from vegetable infusions which had been boiled for ten minutes in a current of air passed through a filter of red-hot porcelain, and sealed up in glass bulbs. On the other hand, Edwards and Colin showed that the seeds of albuminous and starchy plants would not withstand a temperature of 167°, whilst the cells of cellular cryptogams, the yeast plants for example, are destroyed at a lower temperature.

Hence the reasonable, general deduction that a heat short of decomposing organic matter into its inorganic elements, though it does destroy the organisms which are in the process of active propagation, yet it does not destroy the spore, but leaves it to reproduce its kind whenever the proper conditions of pabulum recur. In this connexion, neither the experiments of Dr. Henry of Manchester, published in 1832, nor those of Von Busch of Berlin of 1851, can now be admitted to be as conclusive as they first appeared. Indeed, in the case of the latter, wherein epidemic puerperal fever occurred after six or seven months in wards from which it had been banished by a temperature of about 167°, they rather tend to confirm the view that the active ferment was destroyed, but that the spores were left to regenerate the disease as soon as the proper pabulum should have accumulated.

Heat, then, short of that required for the decomposition of organic matter, namely, about 280°, does not appear to destroy infectious poisons, except so far as it may be, and probably is, concerned in the action of all chemical disinfectants which act by that species of slow combustion involved in oxidation and deoxidation.

This, however, is far from diminishing the importance of the

application of heat in its legitimate sphere as a disinfectant, but should rather serve to enlarge the sphere of its application and stimulate further research.

We have a statement very confidently made by Dr. Elisha Harris, that a judicious system of scalding the infected clothing under his direction, did protect the washerwomen and attendants from the infection of yellow fever, and from the infection of other far more infectious maladies in twenty-four cases of these diseases which occurred in the Floating Hospital under his charge. But Dr. Harris himself is the first to guard us against drawing conclusions from so limited an experience. Since that time (1860) this experience has been extended with great advantage; but the published results met with are given in generalities, or at any rate without the systematic accuracy which should characterize such statements before they can be admitted as established facts.

Similar statements were long ago made, and are now well known, in regard to the disinfection of vessels by dry heat and by steam. Dr. A. N. Bell's account of the steaming of two vessels, and the remarkable results which followed, have been long published and are highly interesting and important, obtaining additional force as they do from instances of the application of dry heat in a similar manner which preceded them. These measures were particularly directed to yellow fever infection, but may in time be directed to cholera, ship-fever, etc., and thus by repetition and confirmation establish their results in the way in which alone they can be admitted as positive knowledge.

In concluding this very unsatisfactory notice of heat as a disinfectant, it appears necessary to say that the grounds upon which the deductions are based, and perhaps the deductions themselves, are about as unsafe substitutes for facts as disinfectants are for cleanliness; and that at present the generally received opinions in regard to heat bear about the same relation to definite knowledge that theoretical controversy does to laborious practical application. Such subjects need more persistent labor and less controversy; and your reporter pleads guilty of having talked much but done nothing, and offers himself in this, as an example of the fallacy of any such basis for knowledge, his only apology being that this least inviting part of his duty was not self-imposed.

Common chloride of lime, or bleaching-powder, as it is called, —the calx chlorinata, or chlorinated lime of the pharmacopœia,— when of good quality, is one of the very best, cheapest, and most accessible of all ordinary disinfectants for popular general uses, and has the great advantage of being already pretty generally known. It may be had in quantity for about seven or eight cents per pound, and well subserves the purpose of a deodorizer as well as a disinfectant. After having partially performed its office in a dry way, it may be moistened into a kind of whitewash, and be applied over a broad extent of surfaces with great disinfectant and cleansing effect. Sprinkled dry over dirty floors, pavements, etc.,

and after a day or two washed off with water, its disinfectant properties for such purposes can hardly be overrated. Its effects are obtained through the agency of the chlorine slowly liberated, and the caustic lime it contains.

In the use of free gaseous chlorine as a deodorizer and disinfectant, it is important that it should be continuously liberated until the effect is obtained, and so slowly, and in such quantity, as not to irritate the air-passages of the inmates of occupied apartments. This object is probably best attained by the formula of Wiggers, as given in Fresenius' Qualitative Analysis. The gas is liberated from a finely-ground mixture of common salt and binoxide of manganese by means of sulphuric acid, the strength of the latter being so adjusted by dilution that the elimination is effected without the application of heat, and is neither too rapid nor too slow. Then, a definite quantity of both ingredients being taken, the effect becomes easily controlled. In the early part of the late war, your reporter, while serving on a Board of Revision for the Army Supply Table, by a slight modification of Wiggers' formula, and a little adaptation to the particular objects in view, succeeded in rendering this agent both manageable and convenient for hospital use; and the enormous quantity used during the war seems to indicate that in practical application the method did not disappoint the expectations of the Board. Indeed, the plan may be said to have come into such effective general use, that the materials may now be purchased from various manufacturing sources, and are generally accessible. Good binoxide of manganese, containing seventy to seventy-five per cent. of the oxide, can generally be had in all the principal markets. That imported from Saxony is by far the best, but our own native oxide may be used if the best qualities be selected, and the proportion of it be increased about one-third. Ashton's dry "factory-filled" common salt, and good Saxony manganese, are ground together in equal parts into a very fine powder. This forms what is called the "common salt mixture for chlorine," and can be supplied in large quantities at about fifteen cents per pound, but in small quantities would cost nearly or quite double that estimate. The other element is the "sulphuric acid mixture for chlorine," and is simply concentrated sulphuric acid, or "oil of vitriol," diluted with water until it has a specific gravity of about 1.51 at 60°. For this strength, forty-five parts of strong acid and twenty-one parts of water are required, and the mixture must be made in a vessel not liable to be broken by the sudden heat developed by the mixing. This acid mixture must be allowed to become perfectly cold before mixing with the powder for use. The sulphuric acid mixture can be supplied in large quantities at about four cents per pound, but in small quantities at not less than six cents, and each pound measures nearly ten fluidounces. It was experimentally determined by your reporter, that 195 to 200 grains of the common salt mixture, and half a fluidounce of the sulphuric acid mixture, well stirred

together, liberate within twenty-four hours about fifty-seven cubic inches of chlorine gas; and that this quantity when gradually diffused through a space of about 660 cubic feet, at no time caused pulmonary irritation or discomfort. This was therefore adopted as the convenient portion or dose which should be normal to two beds' space in hospital wards; and this was the quantity directed to be mixed in the bed-pans, and set under each alternate bed every night during the disinfection.

Tin measures were made which would hold, when struck off level full, about 195 to 200 grains of the powder, and a graduated measure served for the liquid. A pound of the powder and eighteen fluidounces of the liquid, costing from twenty-two to forty cents, give thirty-five of these portions, or enough for say thirty-five beds, for one week, under ordinary circumstances of hospital use. During epidemic influences, or with bad ventilation, much more frequent application would be needed. Besides this form, the materials were supplied in a much neater, more convenient, though more expensive form, put up in strong pasteboard boxes. One side of the box was furnished with a drawer, in which were placed 130 of the above portions, each folded in a paper, and these papers held together in bundles. The remainder of the box was occupied by a strong half-gallon, glass-stoppered bottle, containing sixty-five fluidounces of the sulphuric acid mixture duly labelled. The box itself also contains a label plainly stating the contents and mode of preparing them, and also detailed directions for use. A set of these labels are given on pages 80 and 81.

This formula and method once established, and freely published, as it was, from its very extensive use in the army, was soon adopted by manufacturers of army supplies outside of the Government laboratories, and thus came into somewhat general use. Upon this experience in application it is still believed by your reporter to be the best method of rendering chlorine available for disinfectant use. These boxes of "materials for chlorine" are sold by manufacturers at from $1.80 to $2.00 each, and by the druggists at $2.50 to $3.00 each.

Chlorine controlled in some such way as this, becomes one of the most convenient and manageable, as it is one of the most effective disinfectants for many purposes. In time of epidemic cholera, or at any time when the dejections are pernicious or offensive, the portion or dose should be mixed in the close-stool or bed-pan, and be spread out as much as possible over the bottom of the vessel. Here it is allowed to stand, slowly giving off its gas, and fumigating the bed and furniture around the patient, until the vessel is required for use. It will have then given off its gas in proportion to the length of time it may have been standing, and the remainder will be retained in the residuum for utilization when and where most needed, namely, in disinfecting the dejections, and the sinks, waste-pipes, sewers, etc., into which the dejections are emptied. When the dejection is passed into the

FOR THE OUTSIDE OF THE WRAPPER OF THE BOX.

CHLORINIUM.

The Materials for preparing Chlorine as a disinfectant for Hospitals, etc.

Prepared by

FOR THE BOTTLE OF DILUTED SULPHURIC ACID.

CHLORINIUM.

THE SULPHURIC ACID MIXTURE.

Take of Sulphuric Acid S. G. 1,845.....45 parts,
Water.....21 parts,

Mix them carefully, and when cold put the mixture into strong bottles with accurately ground stoppers, each bottle to contain sixty-five fluidounces.

Half a fluidounce of this to be used for each package of the Common Salt Mixture.

Prepared by

FOR THE FRONT OF THE OUTSIDE OF THE PASTEBOARD BOX.

CHLORINIUM.

THE COMMON SALT MIXTURE.

Take of Common Salt, well dried.....1800 parts.
Binoxide of Manganese, containing 72 per cent.....1875 parts.
Grind them together into a fine powder and put the powder up in packages containing about 195 grains each, and put 180 of these packages in a pasteboard box to accompany the Sulphuric Acid Mixture.

Each of these packages requires half a fluidounce of the Sulphuric Acid Mixture, and yields about 57 cubic inches of Chlorine. This quantity, when thus liberated gradually in a space containing about 20,000 times its volume of air, is borne without inconvenience by persons generally, and is not injurious even in pulmonary diseases. As very much depends upon the ventilation of apartments wherein it is to be used, no absolute rules of application can be laid down, except that it should never be used in such quantities as to produce discomfort or bronchial irritation to patients.

DIRECTIONS FOR USE.

One of the above packages or papers of the Common Salt Mixture placed in a saucer or plate and thoroughly mixed with half a fluidounce of the Sulphuric Acid Mixture, is to be placed under every alternate bed at night, and allowed to remain there three days. Upon the second night the beds which were omitted the first night should be supplied in the same way, and for the same length of time; and the whole process repeated at the end of three days or sooner according to circumstances. Should the wards be badly ventilated, or contain many sloughing wounds, or be subject to epidemic disease, or low forms of fever, the process should be continuous, that is, the mixtures should be renewed every third day. Otherwise once or twice a month may be sufficient. And, when thorough cleanliness and ventilation are attained, the process is unnecessary for occupied wards.

In disinfecting unoccupied Wards, Water Closets, Latrines, etc., by Chlorine they should be first cleansed, be closed up as perfectly as practicable, and two packages used for each 600 cubic feet of space.

Prepared by

vessel, the residuum being thick and tenacious, will, in order to secure the best disinfectant effect, require stirring up to incorporate it with the dejection. This may be done with a short stick kept in the close-stool or bed-pan for this purpose (and to mix the chlorine materials with), the stick to be held in the hand while the vessel is receiving the dejection, and to be cleansed and returned with the vessel to the bedside, where a fresh portion of the materials is at once mixed and spread over the bottom of the vessel to stand in readiness for the next dejection. The warmth of the dejection, when passed into the residuum, hastens the reac-

tion by which chlorine is set free, and this exactly at the time when it is needed, so that a residuum which may have been standing so long as to have liberated all the chlorine that could be set free at the ordinary temperature of the vessel, is by the increase of temperature at once awakened into new activity, and yields a sufficient additional quantity of the chlorine just at the time of greatest need. The disinfectant, then in a state of activity, goes with the infected matter into the sewer; and as no dejections go into the sewer without the corrective, the water-closets, sinks, and sewer all soon experience the benefits, while the rate of disinfection goes along parallel with the necessity, increasing or diminishing with the number of dejections Besides this, as the dejections increase in frequency in any given case, the residues are used in quicker succession, and while containing more chlorine, and this without reducing the quantity liberated in the apartment. The reactions by which the chlorine is set free in this process, form sulphate of soda, and sulphate of the protoxide of manganese, which, with the excess of sulphuric acid, are all actively disinfectant, as well as the chlorine; and the whole result is, therefore, utilized with about as little trouble, expense, or complication, as can be reasonably expected from the use of any similar agency. Indeed, it can hardly be possible that this plan, in practical application, if well carried out, should not yield excellent results for all such special uses; and a prominent quality inherent to it is, its susceptibility of being easily contracted to the scale of any ordinary sick-chamber, or expanded to fit the greatest emergency, by the exercise of but a little common sense.

If, however, as too often happens, the successful application of any agent in its proper sphere be allowed to mislead in the direction of applying it to more general or less appropriate uses, the best interests involved are sure to be sacrificed to a corresponding extent thereby. Hence this plan, though well adapted here, would be entirely inadequate to very many of the other uses of disinfectants—thus well illustrating the important fact that the discrimination necessary to a successful application of the various well-known means of disinfection, is often the only thing wanting. It has come within the observation of your reporter to see attempts at disinfection by chlorine, which through a want of this discrimination were nugatory, though troublesome, expensive, and even dangerous; where a good smoky wood-fire, made in any reasonable way at the point where the chlorine was liberated, would have been far more effective, and cheaper, more simple, and much more easy of application.

Indeed, for the disinfection of empty tenement-houses, hovels, stables, cellars, etc., under many circumstances of not infrequent occurrence, there are few more effective agencies than wood-smoke, diffused by the heat of the fire from which it is produced. This smoke carries carbon, creasote, pyroligneous acid, carbonic acid and oxide, and other products of the imperfect combustion,

with watery vapor enough to dissolve or suspend them, and with heat enough to secure their rapid diffusion and safe transportation until condensed by contact with the cooler surfaces upon which they are finally all deposited in one form or another. Every part of the air-space is thus invaded, and every surface receives its due share of the condensed product with that certainty which characterizes the operation of the natural laws involved.

A good smoking, followed by a good whitewashing, is as thoroughly disinfectant and antiseptic and curative to all surfaces and objects to which it can be thoroughly and properly applied, as the same treatment is to a ham; and there is no more reason for omitting to use such treatment when appropriate, than for curing hams by ozone.

A very large proportion of the uses for disinfectants, however, cannot be well subserved by either of the agencies yet alluded to. Damp, underground apartments, alley-ways, cesspools and privy-sinks, empty or full, sewer-traps, water-closets and urinal basins, garbage receptacles, dirt-piles, and, in short, most of the insalubrious places not accessible to copious washing or other cleansing processes, are often best disinfected by some drying and absorbent substance or powder; and if such substances possess chemical or physical properties whereby the sources of insalubrious effluvia are changed, at the same time that their emanations are absorbed and fixed, so much the better. Prominent among such substances stands lime—common quicklime. A dark, damp, mouldy, ill-conditioned, bad-smelling room or closet may be rendered comparatively harmless by keeping in it a bucketful or two of lump lime, and renewing this as it falls to powder; and in this way, or when powdered, or in a liquid form, as whitewash, lime is perhaps as susceptible of important uses as a disinfecting or cleansing agent as any other substance known. As an absorbent of aqueous vapor it stands preëminent; and when it is remembered that the aqueous vapor of the atmosphere is the great solvent and vehicle for carrying noxious effluvia and decomposing organic matter, it will not be difficult to understand an important characteristic of lime as a disinfectant. To its affinity for water, for carbonic acid, for sulphur compounds, and for acids in general; to its caustic nature as destructive of the lower orders of organic life; to its saponifying action upon all oily or greasy matters, and to its slow solubility whereby its action is rendered persistent and durable, does lime owe its chief efficiency as a disinfectant; and these characteristics duly considered, will lead any mind of ordinary intelligence to the proper indications to its uses.

One of the great drawbacks to the very free use of lime at present is its very high price. Good lime cannot now be had at less than $2.30 per barrel in large quantities, or $2.40 in small quantities of one to five barrels. The barrels are small, and contain from 190 to 230 pounds each, averaging about 205 pounds, making the prime cost nearly one and a quarter cents per pound.

Then, as it is by far best adapted to disinfectant purposes when in the form of powder, it will cost say not less than one cent per pound additional for porterage, grinding, and repacking. This repacking must be done in other barrels—say second-hand flour-barrels, because the original lime-barrels are worthless to carry a powder in. This, again, increases the cost by about one quarter cent per pound; so that the neat cost of lime properly ground, and put up for sanitary purposes, would be about two and a half cents per pound, each barrel of the powder containing about 300 pounds. If done upon a very large scale, or at the kilns where the lime is made, this estimate would include a sufficient manufacturer's profit, and still secure the proper quality of lime. But in any moderate quantities, with due regard to the proper quality and management, another quarter of a cent per pound would have to be added for manufacturer's profit, making the net cost say two and three-quarter cents per pound, or $55 per ton, including barrels, cooperage, and the expenses of delivery. Thus prepared, it might with utility and propriety be distinguished as Sanitary Lime—meaning, of course, lime well adapted to sanitary purposes. Well put up in paper-lined barrels, and stored in a dry place, it would probably keep well for some time, without risk of bursting the barrels. For many uses the disinfectant value of such a powder could not be excelled, nor its place be as well supplied by any other agent.

Another disinfecting agent of somewhat similar character, and scarcely less important, is found in common wood charcoal. Lowitz was among the first to demonstrate its peculiar effects upon offensive smells; and long before the researches of Stenhouse, its properties were pretty well known. It is a most powerful and wonderful absorbent of almost all the offensive and noxious gases, and, unlike lime, performs its office as such, for the most part, independent of aqueous vapor, though a good absorbent of this vapor also. It packs away the gases within its pores in some unknown way, so that it is capable of taking up and retaining, under favorable circumstances, fifty to ninety times its volume of some gases; but under the ordinary circumstances of practical use this absorbent capacity is very much reduced. The absorbent properties of charcoal are generally attributed to the effects of surface adhesion (which, in itself, is hardly more than an expression of ignorance). For instance, Mitscherlich calculated that the cells constituting a cubic inch of boxwood expose a surface of not less than seventy-three square feet. Now when this boxwood is converted into charcoal, by driving off everything except the intractable carbon, this latter, since it does not fuse, retains the cellular structure, and becomes a perfect skeleton of the wood, leaving the enormous cell-surface not materially diminished, but freely accessible to outside contact of gases and vapors through the channels opened by the volatile portions which escape in the process of charring. This seventy square

feet of surface absorbs, attracts, or condenses different gases in different proportions or quantities, but, singular to say, in very nearly the proportion of their solubility in water. It also attracts them in groups as well as singly, and free from aqueous vapor as well as combined with it. But it is a fact very damaging to this surface-adhesion theory, that the quantity of at least one gas thus absorbed is so great, that if reduced to the liquid state by pressure, its volume as a liquid would exceed that of the charcoal which would absorb it. Hence, the ultimate atoms of such gas must be nearer together in the pores of the charcoal than they are in the liquid state—a circumstance difficult to realize and impossible to explain.

It is, however, not only as an absorbent that charcoal performs its disinfectant functions. In common with spongy platinum and some other substances, it has the power of inducing chemical action by increasing the natural affinities of certain elements; and its prime quality in this respect, and that upon which its disinfecting properties probably chiefly depend, is that of oxidation, or a combustion with heat, but without light. It appears to store up the oxygen in its caves and pores, nourishing up this giant into that condition of activity which is so peculiar and characteristic as to have required the new name—ozone. This active oxygen (nascent oxygen or ozone) seems to lie in wait, hidden in the pores and recesses of the charcoal, and whenever any weakly or loosely-compounded gas or vapor comes along, invited or impelled by the absorbent power of the charcoal, the greedy giant pounces upon it and gobbles it up. Thus sulphuretted hydrogen, absorbed by charcoal, has its hydrogen at once seized by the oxygen and converted into water, whilst the sulphur is precipitated in a harmless condition, or oxidized into sulphurous or sulphuric acids. Carburetted hydrogen is decomposed and oxidized into carbonic acid and water; and such reactions probably continue by fresh accessions of oxygen from the air without, until the capacity of the charcoal for the gases and mixed products is saturated.

Then, as far as disinfection is concerned, the charcoal has returned to the condition of wood, its cells being refilled with volatile matters, and requiring reheating to renew its powers. In all these wonderful and inscrutable effects, the charcoal itself appears to undergo no change nor to suffer any waste. However effective it may be, it is but passively so, preserving that unchanging indestructibility illustrated in the carbonaceous inks of Herculaneum, which have withstood the vicissitudes of two thousand years. In whatever way the action of charcoal as a disinfectant and antiseptic may or may not be explained, the truth that it is so, to a most remarkable extent, is both well known and well established through that safest and best of all teachers—time. Dr. Ure mentions that oak stakes were found in the bed of the river Thames, which, on the authority of Tacitus, are shown to have been placed there probably by the Britons when defending them-

selves against an invasion of Julius Cæsar. The stakes had been charred on the surface, and had thus not only retained their form, but the charring had perfectly preserved the wood within in a firm, sound condition. Almost the entire city of Venice is built upon surface-charred piles; and your reporter slept very soundly over the heads of these old piles, with more confidence in the antiseptic powers of charcoal than is common in these days of novelties. Thus the history of ages long past is both written and preserved for us through this agency, and the monuments and architecture which verify and illustrate this history are themselves based upon this same antiseptic power. It seems not a little curious then, that it should be considered useful to rehearse and advocate the antiseptic and disinfectant properties of charcoal to an intelligent profession at the present day, as if they were new or strange; or as if your reporter could add a single atom to the accumulated knowledge of the past. It has been long thoroughly well known that dead animals and vegetables in a state of putrefaction, and vats of organic liquids in a state of fermentation, when covered with powdered charcoal, have their processes arrested and their order of progression entirely changed. In the case of the animal and vegetable matters the disintegration goes on, but the quality of the process is entirely changed. The gases and vapors given off as the mere volatile portions pass away, are odorless and comparatively harmless; while the more fixed and the inorganic portions innocently return to dust. The putrefactive fermentation which yields noxious products is changed by its contact into a slow combustion, which yields the products of combustion only, namely, carbonic acid, water, oxides of nitrogen, and ashes.

This certainly is all that can be necessary to indicate to common intelligence the value of charcoal as a disinfectant. Its peculiar adaptations in use will always flow from a very little reflection upon its properties; and a little cultivation of the knowledge of these properties, will prepare any moderately educated mind to recognise the special indications to its use at the time of need.

Sanitary Charcoal (your reporter proposes this title for it) should be freshly burned—be ground into a moderately fine powder, and be packed in good paper-lined flour-barrels, with the least practicable exposure to air and moisture in the process. It will then possess its maximum ordinary efficiency, and will retain this condition almost indefinitely. In large quantities, common charcoal now sells at about one dollar per barrel, and the contents of a barrel, as sold, will weigh from sixty to eighty pounds, according to the moisture it contains. But when dried for powdering, the same contents will not weigh over forty to fifty pounds. It will then cost not less than one cent per pound to powder it properly, and half a cent additional for barrels, packing, and delivering. This would make a total net cost of

say three and a half cents per pound, to which would have to be added a manufacturer's profit proportionate to the scale of production, say one-quarter cent per pound, making a total value in first hands of say three and three-quarter cents per pound, or seventy-five dollars per ton in barrels of about one hundred and twenty-five pounds each, delivered.

A due consideration of the multitudinous uses for disinfectants, leads your reporter to the belief that many of these uses would be best subserved in special application by some judicious combination of lime and charcoal. This, in the manner and proportion now to be proposed, has never, so far as your reporter knows, been tried in any public way, though the association of the two substances for this purpose is by no means new. The formula used by the British Sanitary Commission in the Crimean war (see Report, p. 144), was one part of peat charcoal, one part quicklime, and four parts sand or gravel; but the character and quantity of the last ingredient seems to indicate, in the absence of direct information on this point, that the mixture was not ground together, a point which, *à priori*, would seem of some importance, since the prominent utility of the association is that the lime and charcoal may keep each other in the condition of greatest efficiency, a point which can only be secured by a very intimate mixture in a state of powder. It may have been that the lime and peat charcoal were powdered together, and then the sand and gravel used as a diluent, since the Commission remark that charcoal loses its effect when wet, while lime gains efficiency by moisture; and the common-sense deduction from this experience would be, that by grinding them together the lime would serve to keep the charcoal dry, and in so doing would itself gain efficiency. However this may be, certain it is that the use of lime and charcoal together, for disinfectant purposes, is neither new nor unsupported by practical application on the large scale; and the mere varying of the proportions, and of the kind of charcoal, and the proposal of a name for the mixture, is but the appropriation of other people's ideas, as soon as they become worth appropriating, in a new dress, and under a new title, with this disadvantage, that the accumulated experience in regard to the old mixture must in a measure be sacrificed to the theoretical, or *à priori* reasoning of the new. An experimental observation, in order to be accurate and reliable, must commence anew upon the new basis. Nevertheless, upon the testimony of the British Sanitary Commission, carefully guarded and equivocal as that testimony is, and upon the circumstance that peat charcoal is not easily accessible to us, and is much inferior in charcoal value to our common wood-charcoal, and lastly upon purely theoretical grounds, your reporter recommends the trial of modified materials in a different proportion from those hitherto adopted; and suggests as a convenient name for this mixture, that of "Calx Disinfecting Powder," or, more briefly, "Calx Powder," the sole aim of which is better to

combine the advantages and cleansing effect of lime and charcoal under a more definite formula and name. The proportion in which the two agents should be mixed must, for the present at least, be determined by a mere act of judgment, and it is upon this basis, and this alone, that the proportion of four pounds of lime to one pound of dry charcoal is advised. This gives a proportion by volume of pretty nearly equal measures, and yields a fine grey powder of a sabulous character, convenient enough for general use.

This powder attracts moisture so rapidly, and with such avidity, and increases so much in bulk by this slaking of the lime, that a bundle of it containing a pound tied up in double paper, in lying for a few days undisturbed upon an office-table, burst the papers, and was found to weigh one-fourth more than when first tied up. The heat evolved in this absorption of moisture is very considerable, and a very important element in the efficiency, since it not only serves to create a constant commotion in the air around the powder, bringing fresh and fresh portions of air into immediate contact with the powder, and discharging this air upwards, dried, rarified, and purified; but this heat also coöperates with the desiccating effect of the lime upon the charcoal, quickening and renewing both its chemical and its absorbent action. Hence, the Calx Powder, in common with the Sanitary Lime and Sanitary Charcoal, must be kept as much as possible out of contact with air and moisture when not in use, and spread out with as great a surface of contact as possible when in actual use. Hence, the surface exposed, rather than the absolute quantity used, becomes the measure of the effect produced; whilst the thickness of the stratum in which they are exposed only increases the effect in proportion as it may be from time to time stirred up for the exposure of new surfaces, until the whole quantity in use is spent or saturated. The economy in management of these agents thus indicated operates both upwards and downwards, cleansing equally the surfaces upon which they rest, and the air above them. It is a cardinal rule in the use of all such disinfectants as these that they should be applied directly upon the sources of infection; otherwise they become mere dryers and deodorizers, expending their force upon the comparatively useless object of disarming and concealing the smoke, while the fire is left to burn on.

Should this Calx Powder be found to combine the useful properties of its components with the expected advantages over their separate use, it may probably in time become a suitable counterpart to the common chloride of lime in popular use, and at any rate furnish an efficient alternate free from the odor which is sometimes a serious objection to the chloride. From the smuttiness given by the charcoal, it is however less cleanly in application than lime alone or the chloride, though generally more effective.

This mixture would probably cost, if prepared on the large scale, say three cents per pound or sixty dollars per ton, well put

up in good barrels, and delivered in first hands. This is very considerably less than half the present cost of good chloride of lime, at the same time that it is well calculated to subserve many of the most general and important uses of the latter agent.

A specimen of this powder, but with only about half the proportion of charcoal proposed, is presented with this report; and two barrels of the same powder, containing say three hundred pounds each, are placed at the disposition of the Academy, and will be delivered free of expense wherever the Academy shall direct. One barrel has already been sent to a large hospital where it will be fairly tried.

In conclusion and summary your reporter presents the following propositions for adoption by the Academy:

First.—That cleanliness and ventilation are the only true disinfectants; and that all agencies are disinfectant in proportion as they promote or tend towards cleanliness and ventilation, whether by chemical or mechanical action.

Second.—That mechanical disinfection by scavenging* should always precede all other means.

Third.—That the greatest mechanical cleanliness and ventilation having been attained, the object of next importance is a proper discrimination in selecting that special chemical agency best adapted to the special end in view.

Fourth.—That there are special well known agents in abundance within the compass of the present state of knowledge, that are adapted to all the legitimate purposes of disinfection.

Fifth.—That of all special agencies heat is the most important, because most effectual, and most generally applicable in preventing the spread of infectious diseases by ferments.†

Sixth.—That the special agency of the second rank in impor-

* "Scavenging." This word is coined; and upon Ben Jonson's authority it must not be forgotten that "A man coins not a word without some peril and less fruit: for if it happen to be received, the praise is but moderate; if refused, the scorn is assured." Scavenging is obtained from scavenger, upon the ground that need sometimes justifies the making of a verb from a substantive. Scavenger is an English word derived from the Anglo-Saxon verb *scafan*, to shave, to scrape, and is defined by Worcester as "a person who clears away filth or litter from the streets;" and the definition is given on the authority of Bishop Hall. It is now proposed to go back and form a verb *to scavenge* upon the noun scavenger; and the apology and reason for this is that there is an apparent need growing up sor a single word which will express the simple act of cleaning away filth in the fense of scraping it up, or off, and removing it entirely away, for purposes appertaining to the public health and comfort. The verb *to clean* does not appear to take this comparatively new meaning properly within its scope, since a scavenger does not simply mean one who cleans. For example, it might be said that a house had been well cleaned, but was not scavenged. That is, the filth had been carefully removed from walls, floor, closets, etc., but accumulated in the sinks, sewers, barrels, boxes, etc., from whence the scavenger should take it by a distinct action.

† This word ferments is here used as a substitute for fomites, which should now be given up as inexpressive and incorrect in its application to the propagating cause of zymotic diseases.

tance is chlorine, because most effectual and most accessible for hospital purposes in disinfecting dejections; and the effluvia arising from sources which, from their relations to human life, cannot be positively controlled, as the effluvia from ulcers, wounds, and diseases.

Seventh.—That the special agents which rank next in importance, from the wide scope of their general applicability to the common purposes of life, are first, lime; second, a mixture of lime and charcoal; and third, charcoal.

Eighth.—That lime and charcoal, or any mixture of them, to be well adapted to hygienic or sanitary uses, require a special preparation and preservation which is simple and easily obtained. And that these agents when so prepared may be advantageously distinguished by the names sanitary lime, sanitary charcoal, and any mixture of the two as calx powder.

Ninth.—That among the special agencies for disinfecting empty cellars, tenement-houses, stables, and other confined spaces, the smoke and products of imperfect combustion from a fire of hard or green wood, deserve special consideration from their combined efficiency, economy, and simplicity.

Finally, your reporter begs to offer a word of caution in regard to the already numerous and still increasing number of patented and vaunted disinfectant powders and liquids. These are generally more or less complex compounds of the various well known agents, the prime object of which is to make money easily and rapidly. And this object is not unfrequently sought throug hthe endorsement of bodies like this Academy, and the association of its name with *ad captandum* statements.

All of which is respectfully submitted to your Committee by

EDWARD R. SQUIBB, M.D.

BROOKLYN, May 28, 1866.

Accepted and adopted by the Committee on Hygiene and Public Health, May 28, 1866, and ordered to be presented to the Academy at its special meeting to be held May 30, 1866.

Accepted and adopted by the Academy, May 30, 1866, and ordered to be printed.

DR. GRISCOM, as Chairman of another sub-committee of the Committee upon Public Health, then presented the following:

REPORT OF THE SUB-COMMITTEE

ON

Ventilation.

PRESENTED BY

JOHN H. GRISCOM, M.D., Chairman.

In preparing a report on the subject of Ventilation for the Academy of Medicine, the sub-committee have acted on the supposition that the object of their appointment was merely to present what in their judgment might be found to be the most facile and efficient plans for the purpose, applicable to buildings and situations *of all kinds*, without attempting to argue the necessity of this sanitary measure, assuming that to be fully admitted on all sides.

In seeking for the most available method of ventilation, and one to be recommended for general adoption, wherever this most important of all hygienic necessities is demanded, the committee, before entering upon the practical investigation of the subject, adopted the following points as the standard for their decision thereupon:

First. General applicability to buildings and localities of all kinds.

Second. Efficiency of action, both in the exhaustion of foul air and the supply of fresh air, separately or together.

Third. Capacity for self-action, without attention.

Fourth. Economy of cost and working.

Fifth. Freedom from liability to obstruction, accidental or intentional.

Sixth. Reliability at all times, and under all circumstances of weather, temperature, etc.

Atmospheric air is, like all other substances, subject to the law of inertia, and requires the application of an external influence to put it in motion. In addition, however, to the mechanical forces by which alone solid substances are movable, air is amenable to another motive power—viz. heat.

These being the only motive powers available for the purpose, the employment of one or the other constitutes the chief characteristic of the several methods of ventilation now known.

In circumstances where artificial heat is applicable for the purpose, it is a very useful means for the removal of impure air from

an apartment, inasmuch as a heated column of air, being of less weight than its surrounding atmosphere, must rise and carry with it its mingled impurities. It is thus that a heated flue produces an upward current, whereby any apartment with which it is connected may be ventilated to a degree proportionate to the capacity and the internal temperature of the flue. Thus a common chimney-flue connected with a furnace, a stove, or an ordinary fireplace, is in fact a ventilator; and if its capacity is greater than is needed for the combustion of the fuel, the surplus may be used to ventilate other apartments than the one containing the fire.

Almost every *stove-pipe* may also be converted into a ventilator on the same principle as a chimney. To accomplish this object most effectively, the horizontal section of the pipe should be of rather larger diameter than, and should project a short distance beyond, the perpendicular portion, so as to form a T; the end most distant from the chimney being left open and commanded by a register or valve, so as to be opened or closed at pleasure. When opened, a current of air will set into the pipe (as indicated by the arrow) as long as the pipe or the chimney-flue has a temperature above that of the room.

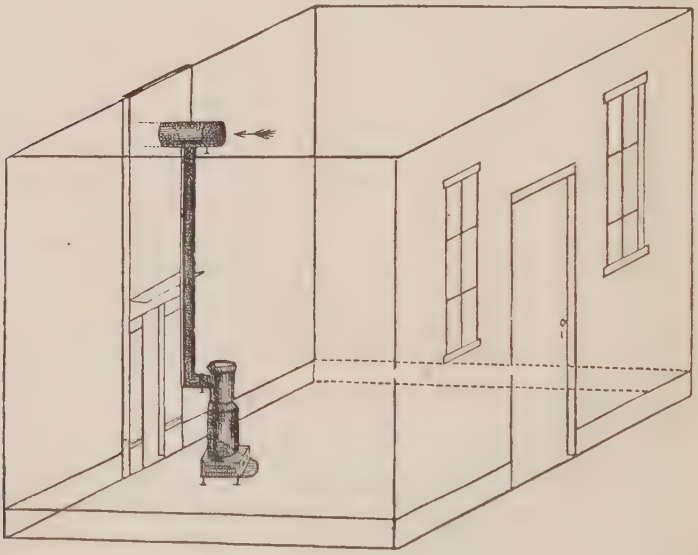


Fig. 1.

This arrangement of the register or valve supersedes the necessity of the ordinary draught-valve, as the opening of the aperture arrests the draught through the stove, and thus dimin-

ishes the consumption of the fuel; and when closed, the current of air through the fire is resumed.

This method is applicable to any stove-pipe connected with a chimney of sufficient draught, and may be used with great benefit in very many of the tenement-house apartments in this city. Especially will it prove useful wherever cooking-stoves are used, such being in operation all the year round.

Other specific modes of applying artificial heat for ventilating purposes have been suggested, two of which will be here noticed. The following description of the first is extracted from a report

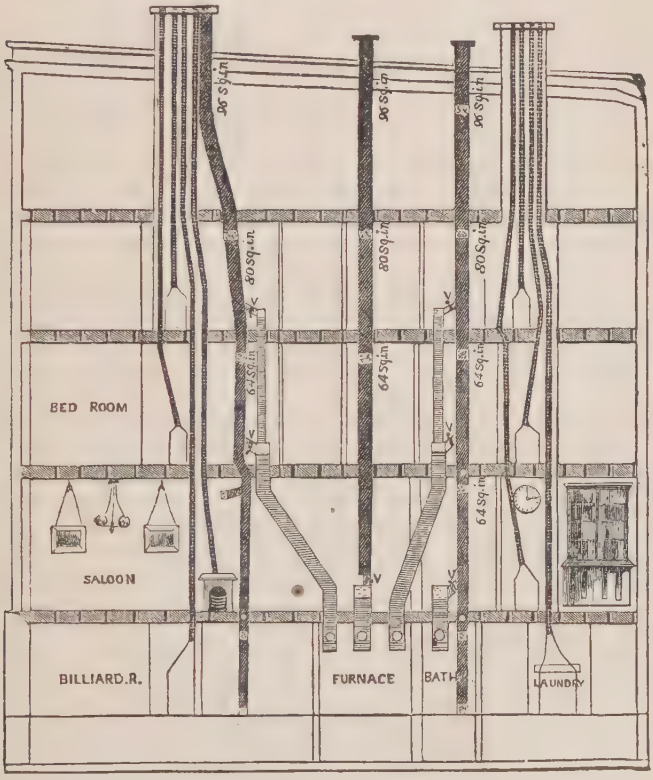


FIG. 2.

"On the Importance and Economy of Sanitary Measures to Cities," by JOHN BELL, M.D., of Philadelphia, article, "Ventilation."*

"The arrangement consists in the construction of independent

* Proceedings of the Third National Quarantine and Sanitary Convention, 1859.

ventilating flues in the walls of the house, in proximity to the hot-air tubes, so that the two may be connected together by means of a lateral or branch-tube, by which a current of hot air may, at any desired moment, be transmitted from the hot-air tube to the ventilating flue. By this means, the ventilating flues, which terminate in the open air like an ordinary chimney, will be warmed by the hot air from the furnace, when the ordinary hot-air register is closed, as at night in a dwelling, or in a school-house after school hours.

"This method has the advantage of being applicable to all edifices warmed by hot-air furnaces of any description, which, in general, are those most needing ventilation. This arrangement may be introduced into many houses already erected, by connecting the hot-air tubes with such of the ordinary chimney-flues as are not used with fire.

"One of the principal advantages appertaining to this plan, is the capability of having a LARGE NUMBER of ventilating flues put in connexion with the furnace. In fact, the number may correspond with the number of hot-air registers, and thus any desirable amount and extent of ventilation be obtained."

This method is illustrated by fig. 2, representing the elevation of the west wall of the residence of E. V. HAUGHWOUT, Esq., Gramercy Park, Twenty-first street, New York, showing the position and connections of the warming and ventilating flues. The letter V. indicates the valve in the connecting tubes; the figures indicate the areas of the ventilating flues in square inches.

Of the efficiency and economy of this method of ventilation (its cost of working being in reality *naught*) we are able to speak from observation of it in several dwellings in this city.

We would here invite attention to a consideration of the *proper shape* of flues constructed for ventilation, for fire-draught, or other similar purposes, which will be found of much importance with regard to the amount of air they will carry away. The greater the size of the flue, the larger will be the volume of heated and rarified air they will convey; but it is an interesting fact, based on sound philosophic principles, that a flue which *gradually increases in diameter* from the lower to the upper extremity, produces a decidedly more powerful effect than one of a uniform diameter throughout its whole length. This truth is practically illustrated in the instance represented in the preceding figure. The increasing size of the ventilating flues (represented by the heavy dark lines connected with the hot-air registers) is indicated by the square inches marked upon them.

The other device alluded to comprises a flue or tube especially arranged for the purpose, in the calibre of which is placed a jet of gas, or other convenient flame, by which the interior atmosphere is heated and rarified, causing an ascending current. This arrangement, known as "Gouge's Atmospheric Ventilator," will be understood by the following sketch and description.

A, glass lantern. B, suction-pipe or flue. C, opening in flue, near the floor, for the admission of air. D, diaphragm for regulating the admission of air to the gas-jet. E, gas-pipe and gas-jet. G, interior flue for concentrating the heated air. H, ascending portion of ventilating flue. I, aperture in flue for admission of heated and vitiated air. J, register for opening or closing the aperture I. K, weather cap.

The advantages of this method of applying heat to ventilation are certified to by numerous persons who have adopted it in their dwellings, closets, refrigerators, and other localities.

While the application of artificial heat for this purpose by the methods herein described, or in any other similar manner, cannot but be regarded as valuable, yet when considered in relation to the six standard points presented at the commencement of this report, it falls short of the position demanded by this very important object. Thus:

First.—There are many buildings and localities in which this motive power of air is unattainable, and many others in which fire or flame of any kind is inadmissible.

Second.—Though a motive power of considerable efficiency, its action is limited to its degree of temperature, and to the *removal* of the air, it being unavailable for forcing into an apartment a current of fresh air.

Third.—Though self-working when once put in action, the source of the heat itself requires constant attention for its steady maintenance, for want of which it may sometimes fail of the needful supply.

Fourth.—In many instances its cost may prevent its use.

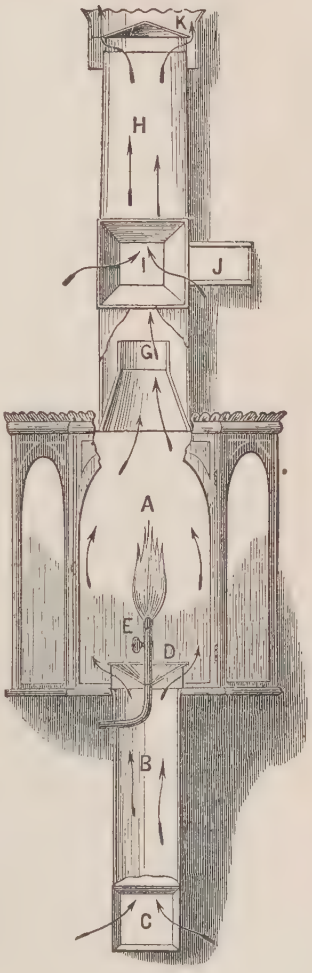


Fig. 3.

The only other motive power to be considered is *mechanical force;* such as the Fan-blower, the Bellows, the Cylinder and Pis-

ton, and other arrangements of similar character. The superiority of these over heat as means of ventilation consists in the greater

Fig. 4.

Fig. 5.
For Ships, Railway cars, and carriages.

Fig. 6.
Chimney with ventilator attached.

power of which they are capable in producing currents both for

the exhaustion and supply of air, and their efficiency under all circumstances. On the other hand, their disadvantage has hitherto been the inability to obtain the power to put the necessary machinery in motion, in many places where ventilation is most desired. Wherever steam is employed, as in factories, steamboats, ships, etc., the apparatus required for ventilation is of course easily applied; but in dwellings, churches, schools, and other places where people most do congregate, its employment is out of the question.

A contrivance for the application of mechanical force for this purpose, adapted to all situations, has been long *the great desideratum*, and until recently such a one has been unattainable; but American ingenuity has at last accomplished the object, so that it is believed we have now the means of securing the thorough ventilation of any apartment whatever, however or wherever situated, and in an unfailing manner.

The instrument here referred to is named the ARCHIMEDEAN SCREW VENTILATOR, a figure of which is hereto appended. (Figs. 4, 5, and 6.)

The motive power of this apparatus is the external wind, which, by an ingenious arrangement, is made to turn a series of Fan-propellers, of a shape similar to those by which ships are driven through the water. By the rotation of these fans the air in the flue (on the top of which the instrument is placed) is lifted up and forced out of the flue, producing a partial vacuum below, which can only be supplied from beneath, whereby an upward current is certainly maintained. This being a purely mechanical power, its action is in no degree dependent upon temperature, or any other modification of the atmosphere; and at whatever time the wind suffices to rotate the machine (and its freedom from friction renders the slightest breeze operative), the exhausting action of the fans is inevitable.

Since our attention was first directed to this improvement, we have learned that an arrangement is in contemplation by which the same motive power will be applied to *forcing in* a current of air. Should this prove successful, this neat and simple invention will meet every requirement, and we shall have a means of ventilation responding in full to every one of the six standard points, viz.:

Universal applicability.
Thorough efficiency both for exhaustion and supply of air.
Self-action without attention.
Costlessness in working.
Freedom from obstruction.
Uniform reliability.

It is with no ordinary satisfaction, therefore, that the undersigned, in conclusion of their duty, are enabled to call the attention of their colleagues and the public to a method of ventilation whose simplicity, economy, and effectiveness, must commend it for general adoption, especially in those places where heretofore

the supply of this great deficiency was impracticable—such as rail cars and carriages of every description, stables and outhouses, where heat is unattainable, sailing vessels, and many complicated buildings, where other means are inaccessible.

To whatever structure a flue, a tube, or an aperture of any kind can be attached, this apparatus may be applied, and its action be relied on.

Besides the essential considerations above described, there are others of no inconsiderable value attached to this instrument. Its neat appearance, and perfect quietness of movement, even when rotated by the strongest winds, adapt it especially to churches, schools, hospitals, and other places where these qualities are required; while its action *being always visible*, gives assurance of the continual ventilation of the apartment to which it is attached.

Jno. H. Griscom, M.D.,
Austin Flint, M.D.

Dr. Stone confessed that this matter of practical ventilation had proved quite perplexing to the Metropolitan Board of Health; that the faulty construction of the tenement-houses had imposed an enormous responsibility upon their engineer, in whose competency they had every confidence. This officer was in a great many instances obliged to content himself with the simple demolition of a window and the elevation of the doors three or four inches from the floor. He would inquire of Dr. Griscom regarding the adaptability of this appliance to any aperture in a roof.

Dr. Griscom replied that the only requisite was a simple puff of wind to move the fans within the cylinder, and that he was thoroughly satisfied with its action in the case of his own dwelling. He had applied it in a position and under circumstances of much difficulty, and a current is now maintained by it through a flue which had never before been ventilated.

Dr. Squibb conceived that, after all, *the when* and *the where* calling for the adaptation of these ventilators, belonged peculiarly to the province of engineers. As far as his understanding of the principle was concerned, he thought the invention acted to a great disadvantage, owing to there being no provision for the partial loss of power. To this charge he thought *Espy & Emerson's* ventilators were not liable. At all events, any of them might temporarily give satisfaction; but he would consider any endorsement of these patented inventions by the Academy as quite unwise, if for no other reason than that they were destined to assume their places "among the things that were."

Dr. Sayre referred to an apparatus exhibited by himself in the capacity of a member of the late Health Board before a convention of physicians assembled at his request, in which the air was mixed through the medium of a diaphragm.

Dr. Stone was of the impression that an apparatus working

upon this principle had been tested by the Park Bank and found to have been unsatisfactory.

Dr. Hamilton had been deceived in the case of a beautiful contrivance, constructed on principles apparently scientific, which, while in charge of the Central Park Hospital, he had applied to the chapel at a cost of $500. This building was held to have been a model for the purpose, but the apparatus turned out a miserable failure.

Dr. Sayre thought that the present Health Board had facilities for testing these inventions which had never been enjoyed by any previous corporation; he would therefore suggest that the same be referred to them for the purpose of being practically tried.

Dr. Stone replied that this idea of ample power was a popular error; that were they to thrust these inventions, etc., upon property-holders, the Board could be made legally liable for damages and the cost of alterations, as well as contingent repairs, in the event of a failure. The practical inquiry, "Who will pay the bill?" would everywhere greet them.

Dr. Griscom, in reply to Dr. Squibb, averred that in *Espy's* ventilator the wind was received in but *one* direction, while in the invention recommended in his report, it was made available from *all* quarters. The *Archimedean Screw-Ventilator* had freed large manufactories of the vapor of steam, which had previously rendered it difficult to distinguish objects at a reasonable distance from the observer; had caused steam to ascend from a pipe in a perpendicular line during conditions of the atmosphere in which the opposite condition had always prevailed; had readily drawn up newspapers attached by a string; and, in spite of theories, had been found capable of being turned by a reflected wind. *Mott's* ventilator was constructed upon the principle of the circulation of the blood, and was a success as far as it went; but we needed something more, which has been here supplied—namely, a mechanical power to lift up and carry forward. He disclaimed any intention to procure the endorsement of the Academy in favor of the invention; he merely aimed to present the principle in its proper bearings, and to stimulate investigations in a most interesting quarter.

The following report was then adopted:

The Committee on "Public Health" beg leave to report that they have had under consideration the report of their sub-committee on disinfectants—Dr. Squibb—and that having approved the same, they recommended it to the consideration of the Academy.

The Committee beg leave further to report that they have considered the report of their sub-committee on ventilation, Drs. Griscom and Flint—presented through the Chairman, Dr. Griscom—and that, on motion, the following preamble and resolution were adopted:

While we recognise in the arguments made by the Committee on Ventilation, as to the value of certain methods reported, much that is ingenious, if not that which is absolutely conclusive, we do not think ourselves warranted in adopting and recommending all the plans proposed, until further experience has more fully determined their utility.

Resolved, That this Committee submit the report of the sub-Committee on ventilation to the Academy, and suggest that the methods of ventilation therein described and recommended, be brought to the attention of the Board of Health, in order that, without unnecessary delay, they may be practically tested.

The sub-Committee on Quarantine, Dr. Harris, not being prepared to report, the Committee have requested him to report directly to the Academy.

JOHN C. DRAPER, *Chairman.*

By DR. F. H. HAMILTON.

The Academy then adjourned.

STATED MEETING, JUNE 6, 1866. DR. JAMES ANDERSON, PRESIDENT, IN THE CHAIR.

Dr. HARRIS, from the Committee on Public Health, presented the following :

REPORT ON SANITARY POLICE MEASURES

APPLIED TO THE PREVENTION AND CONTROL OF

Epidemic Cholera.

By ELISHA HARRIS, M.D.

Anchored in the Bay of New York, only fifteen miles from the Whitehall terminus of Broadway, several emigrant passenger steamships from Liverpool, having on board a great number of persons sick and dying with cholera, have been a source of daily comment and public anxiety these many weeks past.

From personal inspection, your reporter affirms the fact that the disease on those ships is the true Pestilential Cholera; while the events that have occurred in those ships since the pest-stricken "*Virginia*" first anchored there more than a month ago, abundantly prove that an infected ship's company, when crowded and at anchor, will, even in the bay of New York, develop all the virulent qualities which this exotic disease has ever possessed in its native haunts on the banks of the Ganges. The hospital hulk "*Falcon*," anchored in the midst of those emigrant pest-ships, has been crowded from stem to stern with the sick and dying; and from its hundred beds for collapsed patients, it has sent, of that class, a large number to their graves daily. The "*Illinois*," the "*Saratoga*," and the "*Portsmouth*," are anchored in the same locality, and have lent their decks for relief and for the transfer and ventilation of baggage. Upon the "*Illinois*," which is a spacious but badly ventilated steamship, when crowded with the uncleansed passengers of the "*Virginia*," the cholera reappeared, and its infection was not abated until one-half the occupants of that ship had been returned to their purified quarters on the recently infected decks from which they came. Concerning the infected ship "*Peruvian*," that recently arrived, the venerable Deputy Health Officer, Dr. Bissell, well asserts:—"Something must be done soon, or the mortality among the passengers of the Peruvian will exceed that of any vessel ever in this port. If we had the '*Illinois*' here, or some vessel to transfer the passengers, it would be far better than to allow them to remain on board." Under date of June 1st, the same

officer stated:* "The hospital-ship is now much overcrowded, every available space is occupied with the dead and dying. We buried thirteen this morning."

These are doings of cholera—as a pestilence—pent-up on a few ships, which the people of New York have declared by their laws as being subject to quarantine restrictions "so long as may be necessary for the protection of the public health."

In the experiences that have been suffered, and that have been studied upon these infected vessels, in the hospital-ship, and in the history of the "*Atalanta*" and the "*England*," which preceded, lessons have been taught which should be heeded, and which we believe are heeded. We will not stop to comment upon them here, but simply remark that when the Honorable Jackson S. Schultz, President of the Metropolitan Board of Health, and Drs. Willard Parker, John O. Stone, J. Crane, the Health Officer of the Port, and Dr. Wm. C. Anderson, resolved that, whatever opposition might confront them, the passengers in those steamships, shut up with pestilence, should forthwith be disembarked, cleansed, fed, and protected on the dry greensward of Staten Island, and that all such unfortunate passengers as may hereafter arrive shall be thus immediately rescued from companionship with pestilence, an imperative demand of hygiene, humanity, and civilization was met in a becoming manner. We may properly mention here, that the problems connected with these apparently simple procedures of the medical authorities of the Board of Health involve the leading questions relating to the prevention and arrest of epidemic cholera.

A pestilence that has killed more than forty millions of people in fifty years may justly demand exact medical researches and the most vigilant and skilful application of preventive measures against its recurrence and diffusion. The stoical fatalism of the Hindoo and the Mussulman naturally regards with serene complacency the sweeping march of this deadly infection. To them it has simply opened the gates of paradise; but to the northern and western nations, until recently, the cholera has been heralded by intense *fear* and *terror*. The resources of art, of military cordons, and of human ingenuity, were tasked to the utmost to erect barriers against the progress of the cholera epidemics. Blindly and barbarously all the appliances and dogmas of the plague quarantines were resorted to by the people of Europe and Western Asia in the two first epidemics.

As all pestilential epidemics are characterized by two essential

* The "*Illinois*" was moved to the Upper Bay and thoroughly cleansed, and several days after the "*Virginia's*" passengers had been taken from her she was again anchored with the cholera fleet. A large portion of the "*Peruvian's*" well passengers were transferred to her. All went well for several days, when suddenly the cholera made its appearance again as an epidemic on her decks.

elements, without which their propagation is virtually impossible as pestilences, the popular mind will more or less intelligently grasp these laws of the pestilential infection and rudely apply them. The *first* law or element of epidemics thus grasped by the common mind is that which relates to *progress of the infectious poison*, or its continued propagation. But the other element or law of propagation is seldom comprehended, namely, *that no epidemic disease becomes generally pestilential except from localizing causes*, which in civilized communities may be *prevented*, and which, of course, *ought not to exist.* Cholera gives us no exception to this law; but, on the contrary, all its epidemics have taught and enforced this great lesson. If, therefore, we lay down the postulate that pestilential cholera is controllable by hygienic works applied to the conditions or causes that *localize* it when epidemic, why need we discuss the subject of quarantines and other restrictions against this disease?

This is the very question upon which the Royal Quarantine Commission of Great Britain stumbled, and which certainly led to some conclusions that are not fully in accord with opinions now strongly urged upon our American councils of science and sanitary control. In the second report of that Commission one of those conclusions is reiterated in the following emphatic terms:*

"We apprehend the question of contagion has no necessary connexion with that of the importation of epidemic disease. The alleged importation of epidemic disease is a question of fact, to be determined like any other fact by evidence. The evidence appears to us to be decisive against the hypothesis of contagion, but because it shows the utter insufficiency of the grounds laid to establish the fact of importation. In like manner with respect to quarantine, if it were admitted that one true and invariable character of epidemic diseases is, that they are contagious, it would not follow that quarantine can prevent or even check their spread. Whether quarantine is capable of exercising any such influence or not, is a question to be determined, like other questions of fact, by observation and experience; and, as we endeavored to show in our former report, the real practical question for the public and the Legistature, is not whether epidemic diseases are contagious, but whether quarantine can prevent their introduction and extension.

"With respect to epidemic cholera, we have shown in our first report, by a body of evidence which has not been impugned, and which is generally admitted to be conclusive, that whether this disease be contagious or not, quarantine has had no influence whatever in checking progress; and that wherever, in the recent course of this pestilence throughout Europe, quarantine was put in force as a measure of prevention, it was speedily abandoned as useless, and even mischievous.

* See first report of that Commission, *Blue-Book*, 1849; and second report, 1852.

"On the first irruption of this pestilence into Europe in 1831–2, every nation successively menaced by it endeavored to bar it from passing its frontier by rigorous quarantine and by military cordons, but in every instance without avail. Again the like attempt was made in 1847–8; and again it was everywhere admitted to be utterly ineffectual.'

In regard to cholera, as in regard to yellow fever and typhus, *American experience*, and, I might almost say, our *New York* experience, has taught lessons so complete and so varied as to render it unnecessary to go far from home for the instruction that should direct our local sanitary measures against these infectious maladies. But inasmuch as your Committee has been directed to report upon the external sanitary police or quarantine restrictions required in the present state of society, and by the present state of sanitary knowledge, the statements now to be submitted will not be based upon our experience and observations in New York and our American States alone. The laws and phenomena of epidemic cholera are world-wide, and our study of them should be equally cosmopolitan. The points to which we would direct the attention of the Academy will be presented in the following order:

I. What is known or reasonably certain respecting the laws that govern the propagation of the cholera poison, considered with reference to the means by which it may be conveyed from place to place.

II. Sanitary police regulations against cholera: Provision for the sick and for infected clothing, etc.:

Objects and period of quarantine detention: External sanitary measures after the commencement of an epidemic: Present condition of the New York quarantine against cholera.

III. Domestic and municipal sanitary police regulations against cholera.

IV. Conclusions reached in this report, and resolutions expressing the views of the Academy of Medicine.

II. PROGRESS AND DISSEMINATION OF CHOLERA.

That the epidemic course of cholera has always been mainly coincident with the chief routes of commercial and personal intercommunication is proved by history and believed by all nations. It is also well known that the epidemics of this disease spread first along the seaside and through the low interior valleys, and that *usually* the steps of epidemic progress are in such an order of regular succession that the popular opinion prevails that the cholera is spread by personal intercourse from town to town. The true significance of this opinion will presently be examined.

Seeing how impossible it is to maintain by the logic of events an exclusive theory of *direct contagion* as the means by which

the epidemics have progressed over the world, a great number of the oldest observers of cholera have accepted, without reserve, the all-sufficient hypothesis of an epidemic wave ever progressing westward from the original habitats of this pestilence. This hypothesis is untenable as an *exclusive* method of accounting for the facts which four successive epidemics have put on record. Nevertheless, there is in the hypothesis this truth, viz.: As cholera epidemics have not been proved to depend *exclusively* upon any portable thing or substance, much less upon any impalpable or palpable *contagium* produced in and by the person sick, this hypothesis cannot be wholly disproved.

Geographical and chronological data connected with the progress of the cholera epidemics have established the following facts, which must give shape to all rational theories concerning the propagation of the disease:—

1st. Cholera epidemics do not, as was once believed, progress steadily by given gradations of distance day by day, and month by month; but, in every epidemic, the fore-front of the disease has rushed forward in channels and by agencies with which trade was journeying, or emigration was then obviously concerned. The earlier and the later records of cholera in the East are chiefly associated with the vast assemblages of religious votaries while outraging every hygienic law in the discharge of their superstitious rites and orgies. In the same regions, armies, navies, and caravans of merchants, the past fifty years, have in some degree shared the epidemic history of the Hindoo votaries and the Mohammedan pilgrims in carrying the poison of this pestilence.

2d. That the geographical course of the cholera epidemics is *not* invariably *from east to west* is proved by the fact that in connexion with (but I do not now say by means of) navies and passenger-ships, cholera has several times been epidemic on the coast and river towns of China. After foreign commerce had opened Japan to the world, even its ports were fearfully ravaged by this East Indian pestilence. In like manner, too, but with less suffering, French and English transports and naval vessels journeyed from Southampton, Marseilles, and Malta, up the Mediterranean, the Bosphorus, and the Black Sea, with epidemic cholera, in the summer of 1854; and in like manner, after pilgrims from the Coromandel coast in January and February, 1865, had served as the *avant-coureurs* of another great pestilence at Mecca, the same epidemic recoiled upon all the routes and towns through which the weary pilgrims made their way eastward, southward, and westward to their homes again. The obvious and popular aspect of the epidemic of 1865, the march of which has now reached our very doors, was westward and northward. But to the provinces of Assyria, Persia, and Tartary, the march was eastward and northward. On the elevated plains of Azarbijan in Persia, while journeying westward from the city of Tabreez, the 8th of September, 1865, a noble and philanthropic American,

the Rev. Samuel A. Rhea, became one of the first victims of the returning pestilence that had followed the homeward trail of pilgrims upon that high road, at an altitude of 5,000 feet above the sea. These facts are mentioned to prove that the course of epidemic cholera is not invariably westward, and that the deviations from a generally westward course are associated with ships, caravans, or armies, as harbingers of that pestilence.

That there is a general tendency of westward progression in cholera is indisputable, but human emigration is obeying precisely the same law; and it is worthy of remark that after repeated failures of the cholera epidemics in 1849 and 1854 to cross the continent from the Mississippi to the Pacific, the Panama route at last was taken, and, in the autumn of 1855, cholera was returned to us at the Staten Island Quarantine by way of Aspinwall, when there was no other cholera within hundreds of miles; and, within ten days, there followed a sharp and sudden epidemic of that disease in all our wards, and which killed two patients before the premonitory diarrhœa was controlled among the hospital population, thus verifying the type of the disease.

3d. That the time for the beginning of an epidemic of cholera does not wait for any independent and world-wide "wave of atmospheric virus" to be borne across seas and continents independently of human agency. Hence we find the epidemic of 1830–2 escaping at once from Astrachan, up the Volga to Moscow, and from Moscow to the Russian armies in Poland, while all the countries in the south and west of Europe remained untouched by the destroyer. But slumbering on the western borders of the continent, though ravaging the Russian capitals all the winter, the ensuing spring it quickly made its way through England, and striking Dublin and Cork early in April, it rode in deadly triumph in one of the first emigrant ships that reached the port of New York the first week in June. The same week the emigrant ship "*Carrick*" sailed up the St. Lawrence with her dying company. After a brief detention of the first cholera ship in our harbor, during which entire secresy was observed at the port of New York respecting its invoice of cholera patients, the infected ship's company was quickly passed up the Hudson river and along westward by barges of the Erie Canal. Cholera appeared upon some of these canal boats, and when past Utica a number of the dead were taken into an open field, and by the friendly aid of the Oneida Indians residing there the bodies were interred. Six of the Indians who had rendered that humane service, and probably made use of the soiled clothing, died of cholera within a few days. The cities of Buffalo, Detroit, and Cincinnati were successively attacked by the epidemic. New York, Albany, New Haven, Philadelphia, and Baltimore, were at the same time suffering the full force of the epidemic.* During

* For the following case illustrating the point we are here discussing, we are indebted to Dr. A. J. Long, the intelligent Health Officer of Whitehall, N. Y

the same period Paris and the whole of southern Europe were being swept by the epidemic. In the summer and autumn of 1854, as we have noticed, the cholera followed the allied armies up the Black Sea, and there continued to make its deadly paroxysmal incursions, until Hygiea disputed the field with this monstrous offspring of foul air.

We might adduce a great variety of illustrations of the *dependence of the course* of cholera epidemics upon the relative position of the chief foci as the points of departure for the cholera poison, as in the invasion of the United States by the epidemic of 1848–9, when the port of New Orleans by chance—wholly dependent upon the course and nature of travel from the European nidus—was suddenly infected in December, 1848; the epidemic planted itself in the crowded steamboat companies, went up the Mississippi as rapidly as steam propelled the boats;* then alighting upon the city of Memphis, at a distance of 800 miles, on the 20th of December; St. Louis, January 5th, at a distance of 1,200 miles; and in Nashville, Tenn., December 27th, 1,300 miles from New Orleans; it infected and killed people in the way it was at that time, infecting and destroying in the city and vicinity of New Orleans, where, before mid-winter, it killed 1,400 persons. The whole Mississippi Valley was thus infected with cholera that year (1849), long before it burst forth in our Atlantic cities.†

In a letter just received, Dr. Long states: "The first fatal case of cholera known to have occurred in any resident of the United States occurred at the Dock, in the village of Whitehall, June 15, 1832, twelve days after the arrival of the pestilence upon the *Carrick* at Quebec. The history of that case I will give you in the words of Capt. G. Lathrop, of Columbia County, N. Y., of the steamboat *Phœnix*, upon which the case occurred. 'Mr. John Larned, of Troy, N. Y., went to Canada on the *Phœnix*, and when going and returning he spoke lightly of the new disease, to which he was exposed in Quebec, used opium and stimulants freely to keep off danger. He was seized with the cholera in the night, when going from St. Johns to Whitehall. He died soon after the boat reached her dock, the passengers and every person on board fled, but with the aid of physicians in the village, the corpse was buried on an island in the lake. Immediately the pestilence spread though the village and killed one hundred and thirty-nine of the inhabitants. In 1849 the epidemic reached this place by the same route, and by the same means.' "In 1854," continues Dr. Long, "the first case occurred on the steamer *United States*, a raw emigrant directly from Quebec. It was fatal. Next day a case occurred on a boat that had just arrived. Thus it has been with cholera in every one of the towns along the lake. It has invariably been traced directly to the arrival of sick persons, or the remains of the sick." Facts like these, observed in a most salubrious mountain region, and in a sparse population, are peculiarly instructive.

* See Wynne's Report upon the Cholera Epidemic of 1848–49 in the United States.

† The fact should here be mentioned that cholera did not instantly become epidemic in each of the cities up the rivers as it did at New Orleans. In Memphis it prevailed scatteringly until March, when it became sharply epidemic. In St. Louis it rooted itself in certain houses and streets, killed only eighty-seven persons in January, February, and March, but in April it took one hundred and thirty-one, and soon had killed four thousand five hundred and fifty-five out of a resident population of about fifty thousand in the epi-

I adduce these illustrations of the coincidence of the course of cholera, and the course of ordinary travel by water, and by crowded thoroughfares on land. From the Ganges to the Red Sea and the Mediterranean, from the Mediterranean and all Europe, the course of trade and of crowded travel is westward. But wherever we find transport-ships, or caravans moving eastward or northward from a district that is infected with cholera, the epidemic seems to spread in that direction also with equal facility. It follows the trail of eastern as well as western travel. It *does not wait* a world-wide and evenly progressing wave-front, that, like a storm, moves steadily onward as a wave of pestilential air; we neither know nor deny that there is some such general agency of the common atmosphere, and we neither controvert nor accept the hypothesis of "*epidemic storms*," or "waves of pestilence." Perhaps the phenomena on which that hypothesis is based are not yet fully explicable. But when they are explained, it is probable we shall be agreeably surprised by the simplicity of all the facts, and the utter absence of any logical basis for such a hypothesis.

WHAT ARE THE MEANS BY WHICH THE CHOLERA INFECTION IS TRANSPORTD FROM PLACE TO PLACE IN CONNEXION WITH TRAVEL AND PERSONAL INTERCOURSE?

Is the infection transported in merchandise and absorbent materials that have not been in contact with the sick or their evacuations—that is, by simple imbibition of the common atmosphere of cholera districts, as we know is the case in the transportation of the yellow-fever poison?

In answer to this we have the most ample testimony from careful observers, the world over, that no such transportation of cholera poison has ever been effected. Ships loaded with every variety of merchandise in packages, railway trains with every product of industry and art, have plied between the infected and uninfected ports and towns in Europe and America; and until some person or persons in such vessels or in such trains have been attacked by cholera, or until garments, *soiled with the evacuations of the sick*, have been thus conveyed from the infected to the heathful locality, neither ships, trains, nor carriages have ever been reported on reasonable evidence to have been the carriers of the cholera infection. It will be recollected that during the many years that cholera was prevailing in the chief ports of India, commerce in cotton and fabrics was incessantly plied between those places and the ports of Italy and England. Genoese physicians ascer-

demic. In the cities of Louisville and Cincinnati the epidemic did not become general until April, though cholera patients were continually arriving at the levées of those cities.

tained that in the three years previous to 1831, 103,376 bales of cotton from India had been received and sold in that city without harm, though these cargoes were known to have come from the very hotbeds of cholera. While cholera was devastating the town and cities of Russia, in the year 1831, 732 ships, freighted with hemp, wool, hides, etc., directly from the Russian and Baltic ports, where cholera prevailed, arrived and delivered their cargoes in the ports of Great Britain, without communicating cholera in a single instance.* The British sanitary authorities could find no evidence of the connexion of the next year's epidemic, though Sir Wm. Pym and the other members of the Board of Health were strict contagionists. Indeed, in the investigation pursued by that Board the question, "*Is cholera conveyed by merchandise and afterwards communicated by infective germs?*" was distinctly answered as follows: "There is no question in the whole range of sanitary police on which so many and such irrefragable facts can be brought to bear, as on this, derived too from the most authentic and recent sources."

Every epidemic has simply repeated all the experience of the former ones in confirmation of this conclusion—viz. that merchandise, ships, cars, and other vehicles, cannot convey the infection of cholera from place to place, unless at the same time they convey the sick, or the discharges from the sick. In this respect the infectious poison of cholera certainly differs essentially from the poison of typhus, which infects by exhalations from the body.† It is plain also that the poison of cholera differs essentially from the infectious virus of small-pox, and the like specific and diffusible contagions, and particularly does it differ from the infection of yellow fever, which, as we well know, can be imbibed by porous substances wherever the atmosphere of a place is charged with that poison; and may, if kept close and unventilated, be thence transported hundreds of miles to regions in which that fever is strictly exotic, and there be reproduced without any aid from the human body.

The design of this report does not make it necessary for us to adduce evidence upon the vexed questions of the personal contagion or the direct communicability of cholera. We must deal with the *results* of experience, and experience has proved the *person* in cholera harmless. But when we cast aside the hypo-

* Report of Sir Wm. Pym and associates, General Board of Health, London, Jan., 1832.

† The cholera poison is strikingly analogous to the typhoid infection in its haunts and the localizing causes of its endemic prevalence, as well as in the sanitary measures it demands. *See Monograph on the "Prevention and Control of Infectious and Pestilential Diseases,"* articles *Typhoid Fever and Cholera,* published by U. S. Sanitary Commission, 1863.

Says Prof. E. A. Parkes, in his excellent Treatise on Hygiene, p. 432, "It (cholera) resembles typhoid fever in being a late summer and early autumn disease. . . . . At the present time the *preventive* measures are almost the same as in typhoid fever."

thesis of contagion, we are compelled to look for the real source of that infectiousness and portability which cholera is proved to possess, at certain times and under certain conditions. Let us consider for a moment what facts best serve to illustrate the peculiar infectiousness of cholera :—

Observation seems to have established the fact that the first group of pestilential cases of cholera, in any locality, are frequently traceable unmistakably to the introduction of a *first case* from some other locality. An attentive inquiry into the history of the beginning of epidemic cholera in more than a hundred places has convinced the writer, that there is no reasonable doubt that in a large proportion of instances, where distinct districts or widely separated towns are visited by cholera, the *first* group of cases are traceable directly to the introduction of a *first case*, or the distinctly recognised cause of *infection* of a first case, from a place where cholera prevailed at the time. The Cholera Committee of the Royal College of Physicians, Drs. Baly and Gull, in an analysis of the outbreak of cholera in 119 different towns and public institutions in England in the epidemic of 1849, report seventy-three as traceable directly to the introduction of a *first case*, and forty-six others as having no discoverable connexion with any such importation.

The conclusions which the experience of the profession warrants us to accept as the truth concerning the infectious diffusion of cholera by human agency may be stated as follows :

1st. In small communities and in cities widely separated, the earliest group of cases is traceable, in most instances, to association with a *first case* that has come from some place where cholera was then prevailing. In great cities it is seldom possible thus to trace the relation of the cases in wards or streets.

2d. That in cold weather, and in places of remarkable salubrity, at all times the recognition of this relation of an outbreak of cholera to such importation is more unequivocal than in places and seasons in which common cholera-morbus is occurring, and in which the history of the first group of cases of epidemic cholera is traced with less certainty.

3d. That the importation of cases of Asiatic cholera into a community distant from the seat of a local epidemic, and at a time when cholera is not very widely epidemic in the world, is seldom, perhaps never, followed by a general epidemic of the malady in the towns into which the sick with cholera have thus been introduced. For example, the epidemic which swept so fiercely in Rostock, in Mecklenburg, and in some towns in Germany in 1859,* did not spread the disease extensively, though numerous instances then occurred in which inhabitants and travellers fleeing from those infected places died of cholera in England and other places to which they had fled. In some of these instances a certain small number of persons who were near to such dying persons

* See Prof. Th. Akermann, on *Cholera in Mecklinburg-Schwerin* in 1859.

suffered the same disease, but no general epidemic ensued. This fact has been fully illustrated at the old Quarantine establishment on Staten Island, where, at various periods, and without any apparent or probable dependence upon an epidemic influence in this latitude, there have occurred ten distinct local epidemics, six of which were strictly limited to the twenty-five acre lot of Quarantine ground.*

4th. That there is good reason to believe that the restraints of a judiciously managed quarantine and external sanitary police may help to *defer the outbreak* of epidemic cholera, when otherwise that pestilence would commit ravages which improved internal sanitary regulations would prevent or at least greatly mitigate. It was possible, we fully believe, to have kept cholera out of the city of Cincinnati, or the city of St. Louis, in 1849, until municipal regulations could have been brought to bear upon the localizing causes which were so rife in the winter and spring of 1849 as to afford abundant pabulum to the first germs of the pestilence that reached the levées and low quarters of town. Before those inviting causes were touched by the municipal authorities, the epidemic had leaped all intervening territory and killed one in every six of the population then in those cities. Last year, and until this spring, the city and surrounding district of Rome, in Italy, maintained a rigid and exclusive quarantine. Its rule was absolute for fifteen days upon every human being, and upon every piece of personal clothing, baggage, and bedding. Rome was filthy; Rome was environed by a malarious district, and the world's travellers were incessantly journeying to and from that city; while several towns within the Papal States were already smitten by the epidemic. For once, says a distinguished American physician, who was witness to the facts, an exclusive *cordon sanitaire* was maintained for more than half a year. In 1865 and 1866 cholera failed to visit Rome. It probably is possible for an absolutely exclusive sanitary cordon, maintained over a radius of thirty or forty miles from the place to be protected, as at Rome, to prevent an epidemic of cholera from reaching an inland and uncommercial city. But the absolute necessities of a commercial population, the best interests of society, and even the interests of the public health, forbid such exclusive restrictions, when necessary that they be long continued or universally exercised. In parenthesis, we would fearlessly declare our convictions that such system of quarantine restrictions would be far worse than cholera itself. But upon this point we offer no dis-

* Every officer who witnessed these outbreaks of cholera believed them to have arisen solely from the cholera patients, their baggage, or their evacuations; and finally the evidence upon this subject was reduced almost to a certainty that the local infection and epidemics depended wholly upon the cause last mentioned. See *Notes on Cholera*, by Dr. John W. Sterling, in *N. Y. Journal of Medicine;* the Annual Report of the Physician-in-Chief of Marine Hospital, 1855, 1856, and 1858; and a *Report on Epidemic Cholera*, by Council of Hygiene, New York, Nov., 1865.

cussion. Let the following fact illustrate what would result in ninety-nine cases in one hundred of all such attempts at exclusive sanitary cordons as substitutes for internal sanitary measures.

In the town of Palma, on the little island of Majorca, during the summer of 1865, the people undertook, by means of the strictest kind of quarantine, to protect themselves against cholera; and on the 3d of September it had selected its first victim in that place, on the 9th it killed forty-three, and on the 10th, forty-eight; and in its course twenty-one out of twenty-nine of the municipal officers had fallen. The measures taken to protect this little town became the means of intensifying the epidemic. The people were suffering *intense mental anxiety*, and great want of food.

5th. The analysis of all testimony that is trustworthy concerning the methods and means by which cholera epidemics are extended, and the cholera poison repropagated, brings us to the conclusion that the two great factors always present, and always coöperating in the spread of cholera, are, *first*, foul air; and *second*, a specific poison, which together maintain a mutual dependence upon each other.

The microscope and all the resources of chemical analysis have failed to present to us the germs of the poison. Ages of researches may, for aught we know, only establish a sort of negative definition of this strange ferment of pestilence. But concerning the nature, sources, and formation of the foul-air poison that serves as the pabulum of the ferment, the medical world already has ample knowledge. Let these words of Simon, the Chief Medical Officer of the British Privy Council, be borne in mind by every Board of Health, and by every sanitary officer:—

"The specific migrating power of cholera, whatever its nature, has the faculty of infecting districts in a manner detrimental to life, only where the atmosphere is fraught with certain products, susceptible, under its influence, of undergoing poisonous transformations. Through the unpolluted atmosphere of cleanly districts it migrates without a blow; that which it can kindle into poison is not there."

6th. The albuminous evacuations ("rice-water" fluids) of the sick with cholera have been logically proved to possess the power, under favorable circumstances, of propagating the disease, and that such reproductions of the poison by an infectious process may occur in a few hours where temperature and moisture and certain local conditions favor. What this fermentation, change, or catalysis is, we do not know. But the results of careful observation have rendered it highly probable that the *only* power of *personal* infection or communicability of cholera resides in the diarrhœal evacuations, which acquire this property by some fermentation soon after being discharged.

As this subject has been so recently discussed in this Academy, and as every person should examine the convincing testimony which Drs. Budd, Pettenkofer, and Thiersch have recorded, we

do not attempt any critical examination of the question in this report. Let us accept the facts, and prudently emulate the Levitical lawgiver in our rules of cloacal disinfection and cleanliness. An effective sanitary police against cholera and every quarantine code should make it a regulation that *the evacuations from choleraic and diarrhœic patients be instantly, always, and completely* DISINFECTED. We should bear in mind that this infectious property of fermenting albuminous discharges from the bowels is not a marvellous thing, for we witness analogous sequences, perhaps upon the same principle, following the exposure or neglect of alvine dejections in dysentery and typhoid fever. Various conditions and elements may conspire to produce lethcal infections, but it is manifestly essential that the fermenting animal evacuations, impressed with some type or change or catalysis, should impart that type and force to the poison which makes it a virus that will perpetually repropagate itself. But there is much reason to believe that neither the "rice-water" fluids nor the cadaver of a cholera patient are independently and strictly infectious; some changes, and some collateral elements of a virus are necessary, as asserted by Thiersch, Snow, and Pettenkofer. This view is strongly corroborated by the universal experience of pathological inspections in the dead-house.* Yet with all his fearlessness of danger in handling the dead body or the "rice-water" fluids, no prudent physician would allow the same fluids and tissues to be long neglected, and carelessly exposed in a warm and humid atmosphere.

II.—QUARANTINE REGULATIONS AGAINST CHOLERA.

(*a.*) An external Sanitary Police and a rational quarantine against cholera, should be maintained during the period that an epidemic is impending, if persons are arriving from sea with the disease. But whenever cholera has fixed itself as an epidemic within the boundaries so guarded, such quarantine regulations may safely be reduced to the simple process of cleansing and disinfection, so far as the epidemic district is concerned; yet as regards emigrants and travellers who are journeying onward, public safety as well as their own welfare requires that they be detained under hygienic care, in a perfectly salubrious place, until it is certain that they are not at the time infected. This detention should be as brief as can be made consistent with the objects here stated. As this kind of restraint could be required only in cases where emigrants and travellers had been, at the time, exposed to places in which the cholera poison was known to be

* The experience of Dr. Jarhurchen, of Moscow, in 1831–2, is the type of all experience in the dead-house with cholera. That physician made careful autopsies in fifty cases of cholera, and neither he nor his attendants received any harm thereby.

concentrated and active, as in an infected ship, it is the opinion of your committee that no detentions, except for cleansing and medical examination, should be required of other classes of travellers during the epidemic.

The requirements for an effectual *quarantine* against cholera, so far as such restrictive measures are either practical or desirable, are briefly these:

1st. A place for cleansing, by the water-side or over a flowing or tidal stream, where the sick and all suspected persons may be received and cleansed without exposing any of the inhabitants of the locality to the sick, and without contaminating the soil or drinking water of the inhabited district with the ejected fluids of cholera patients.*

2d. The immediate disembarkation and thorough cleansing and disinfection of all persons, baggage, and clothing that may reasonably be suspected to be infected. The necessary appliances for accomplishing these objects are simply the place or places for the sick, the suspected, and the well persons; the means of cleansing by washing; the means of instant disinfection by boiling or by gaseous fumigation of infected things. This is all very simple, and requires no expensive structures, no artificial islands, no expensive machinery. Properly managed, all this work of purification might safely be done on any shore a hundred yards remote from the resident population. That it might, for small numbers of persons, be equally well accomplished on floating hulks cannot be denied; but these would not be adequate to the necessities for personal cleansing, etc., where thousands of persons are concerned. Just here lies the whole difficulty in our cholera quarantine in the lower bay. There is but one hospital-ship, and three ill-adapted old ships, to serve as reception and cleansing hulks.

(*b.*) The rule for detention of persons liable to an attack of cholera, should not be an inflexible one; but as the chief design of all detentions is to effect a thorough purification, the period of detention for the possible development of the disease should not be protracted much beyond the time requisite for cleansing and satisfactory medical inspection and inquiry.

But in any case, where the circumstances attendant upon quarantine detention are unfavorable to the health of the persons detained, the period of such restraints should be as brief as the public safety will justify; for a single patient, or single bundle of infected clothing, in a filthy and crowded place, would strongly tend to propagate a pestilence that quarantines could not restrain. The period of quarantine detention, therefore, must

* The reasons for advising that quarantine or cleansing in cholera should be performed by the water-side, or over or upon the water, were stated by Dr. Baly in his Report to the Royal College of Physicians, in 1854, and by the author of these pages in a Report to the N. Y. Commissioners of Quarantine, 1859. Dr. Marsden and others have since given similar advice.

depend equally upon the actual accomplishment of purification, and upon the avoidance of the perils of crowding, concentration, and the increase and diffusion of pestilential poison when purification is delayed or unaccomplished. The rule or period of detention must be left to a rational discretion.

Varying circumstances must give different answers to such questions. The season of the year, the numbers, the sanitary condition, and the destination of the travellers or emigrants; the feasibility of daily surveillance of the suspected persons, and the families and places to which they go; and, most essential of all, the sanitary condition and sanitary police of the place to which they go, when free, must govern the decision of sanitary authorities in all that relates to detention of persons in quarantine beyond the time necessary for thorough cleansing. The proper regulation of detention for the port of New York might not be the best rule for the ports of Boston, Providence, or Philadelphia, though the *principle* would be the same.

(*d.*) The rule of detention agreed upon by the International Quarantine Conference in Paris, in 1851, was *five days* from the arrival or from the occurrence of the last case of cholera in the ship or company arriving; and with an addition of a like term of *five days* upon every fresh outbreak in such vessel or company. The rule recommended by the General Board of Health in Great Britain, in 1832, when Sir Wm. Pym was Superintendent of Quarantines, was *ten days'* detention as the *maximum*, and five or six as the minimum for persons not sick; while for the convalescents from cholera, *twenty days* was the maximum, and for persons suffering the premonitory diarrhœa, eight days after recovering was the period prescribed. Baggage and clothing were subjected to three days' ventilation and cleansing, and suspected ships and cargoes to a maximum of fifteen days. But since 1848, or, more properly, since 1833, after the British public had witnessed the ineffectiveness of its own and the Continental quarantines, the Royal Commission for quarantine was led to propose in 1849 "the entire discontinuance of the existing quarantine establishments, and the substitution of sanitary regulations.* That proposition led to the immediate overthrow of the entire quarantine system in Great Britain, the Privy Council retaining the power, and possessing the requisite organization for ordering special quarantine or other sanitary measures when necessary, as provided by the Quarantine Act of Geo. IV. But in England, as in the United States, the recent outbreak of cholera has awakened a renewed demand for the establishment of rigid quarantines, and again, for the hundredth time, the fact was boldly announced by officers of the government, that the best *security* must be found in improved sanitary regulations,

* See the *First Report of British Quarantine Commission* and Blue-Book, 1849; p. 127.

such as increased cleanliness and ventilation, and the better supply of water."*

In 1848–9, 1854, and 1859, when cholera was threatening the sea-coast towns of England, this abiding confidence in local and general sanitary measures as the best means of protection against the epidemic, remained unshaken. At the present moment this is the strong basis of public confidence that cholera will not again ravage the British cities. This fact is happily illustrated in a letter which your reporter has just received from the Local Board of Health in the city of Hull, which is exposed to all the perils incident to its daily commerce with Rotterdam and various infected ports at the present time. The language is as follows: "There are no quarantine regulations at the present time enforced against the introduction of cholera; the sanitary arrangements of the Board, now in force, being thought sufficient for all present purposes."

This appears to be a wise decision for a small and clean city that enjoys the high health of Hull, where at the present time the annual death-rate is only 15 to the 1000. But let the fact be here mentioned, that the Privy Council of the kingdom instantly gave full powers of quarantine authority to the Local Board in Hull, upon hearing that cholera was prevailing in Rotterdam, and that for a few days that authority was exercised until the town could be put in a good sanitary condition. But it is a most notable feature of the quarantine orders of the Privy Council and General Board of Health of Great Britain, that it is enjoined upon the local sanitary authorities that they shall forthwith examine all passengers, etc., "and *shall* permit the well persons to land immediately."†

The local Board of Health in that city, as in Liverpool and London, availed themselves of the powers thus granted until the sanitary condition of the city could be put right.

The period of incubation, if there be such a period in the reception and operation of the cholera poison, has not hitherto been deemed worthy of very exact quarantine regulations. We have seen that *five days* was agreed upon by the Paris Confer-

* See *Debates in House of Commons*, May 11, 1866.

† An order was issued by the Privy Council, May 7, 1866, to enforce quarantine regulations at the port of Hull, in language as follows:

* * * * *

"The local authority shall forthwith cause all persons on board to be examined by a physician or surgeon, and shall permit all such persons to land immediately who shall be certified by such physician or surgeon to be free from such disease (cholera).

* * * * *

"The clothing and bedding of all persons who shall have died or had an attack of cholera on board such vessel during her voyage, either at any foreign port, or on shore at such port, or on her passage to the United Kingdom, shall be disinfected, or (if necessary) destroyed, under the direction of an officer of the customs."

ence; *ten days* advised as the maximum term for it in the final Report of Sir Wm. Pym's Commission, in 1832; and at last, the World's Conference, at Constantinople, in 1866, has defined the maximum time for detention as *fifteen days*. Yet in April, this year, before the cities of New York and Brooklyn had been put in a safe sanitary condition, your reporter, with but a limited confidence in quarantines, advised that at *that time* the cholera ships and infected passengers in the bay of New York should be detained twenty-two days after the last appearance of the disease among them: this to afford time to cleanse the cities of their filth, upon which the cholera infection would greedily feed and grow. As great changes have been wrought in the sanitary condition of the cities of New York and Brooklyn the past two months, it would not be unsafe to reduce the period of quarantine detention to *ten*, or even to *five* days, with thorough cleansing of persons and baggage. Better still would it be if our cities were kept in so good a sanitary condition that there need be no fear of this quickly acting factor of pestilential infection which is brought by the sick and their clothing. Such a desideratum is undoubtedly attainable.

With an intelligent comprehension of the facts herein set forth regarding the proper uses and ends of external sanitary police regulations, and certain quarantine restrictions against cholera, the error of substituting such regulations for sanitary works, and for the duties of municipal and domestic hygiene, would not again be committed.

III.—DOMESTIC AND MUNICIPAL SANITARY POLICE AGAINST CHOLERA.

Dwellings and Families, how protected against Cholera during an Epidemic.—Applying the laws that have already been tolerably well ascertained respecting the agencies concerned in the propagation of Cholera, the principles of domestic protection would be enumerated as follows:—

1. Absolute cleanliness in every apartment, place, and area pertaining to the dwelling.

2. Perfect drainage and sewerage of the premises.

3. The most complete dryness that can be obtained by dry cleaning and constant ventilation within doors, and by the use of such absorbents as quick-lime and charcoal, in basements, vaults, stables, and court-yards.

4. The complete sewerage and daily flushing of all water-closets and privies, or the daily and effectual *dry curing* (disinfecting and absorbing) of all privy soil. The daily disinfection of all these cloacal conveniences by means of sulphate of iron or the proper preparations of chlorides, coal-tar, charcoal, etc.

5. The instant and vigilant control of all that pertains to the

sick-room, particularly as regards the dejections, disinfecting immediately. Let ventilation, cleanliness, and absolute disinfection be carried out every moment in the sick-room.

6. The provision of pure water supply, and especially, the avoidance of drinking water contaminated by soakage from privies or cesspools. The use of permanganate of potassa, a few grains to the gallon, whenever the drinking water is suspected to contain undue organic matters.

7. Instant medical and hygienic care of every case of diarrhœa and gastro-intestinal disorders.

8. Systematic philanthropic efforts to provide the homes of the poor and ignorant with all the benefits of domestic sanitary care here mentioned.

CITIES AND TOWNS, HOW PROTECTED AGAINST CHOLERA.

With a pestilence impending, the municipal or local Board of Health should to the utmost degree facilitate and enforce the rules of *domestic hygiene* herein set forth. The same principles should be rigidly applied in every place within cities and villages where cleansing, scavenging, ventilating, draining, sewering, drying and disinfecting are required for the public protection.

There are special and peculiar duties also, which the municipal or town authorities should promptly assume. We may here mention some of them, with brief reasons, as follows:—

Early and ample provision for the proper care of the sick with cholera among the poor, the ignorant, and the friendless. By the same agencies all classes in the community should be advised concerning the proper means of self-protection against cholera.

House-to-house inquiry and advice is the most effective plan for arresting an epidemic and controlling it, whenever it has gained foothold in a populous place; for not only does the treatment of the premonitory choleraic diarrhœas save ninety-nine per cent. of the patients, but by that medical care and the hygienic management (instant disinfection) of the intestinal discharges, one of the active factors of the epidemic infection is thoroughly controlled. The evidence upon this point is remarkably unambiguous and strong, and it invites universal attention. As this is a subject of momentous importance, let us advert for a moment to a few of the points in this evidence:—

(*a.*) Experience in house-to-house visitation in England, in 1849 and 1854, proved that by early resort to that plan of arresting choleraic diarrhœa, the epidemic was itself terminated in particular localities. In Bavaria, in 1854, and with a well-directed use of disinfectants for choleraic stools and privies, the house-to-house inspection arrested the epidemic in certain localities.

(*b.*) Experience has shown that by instant control of the hygienic police of the first groups of cholera, and all that pertains to and surrounds the sick, there is good reason to believe that an epidemic may be arrested, particularly as regards particular streets, houses, and public institutions. Upon this point we would cite the testimony that has been well presented by Dr. Wm. Budd, of Bristol,* Pettenkofer and the Bavarian Sanitary Commission. We may also add the testimony of experience in the British army in India the past three years. In the year 1862, Sir Hugh Rose, the Commander-in-Chief in India, having received the advice of a select medical council, issued an Order for the sanitary police against cholera, of which the following is an abstract:—

"3 On the outbreak of cholera in an epidemic form, either in neighboring villages or cantonments, officers commanding stations will be prepared to move the troops into the selected camp (and under canvas) on the shortest notice.

"4. As soon as any case of cholera is reported on the station the troops will be moved into camp, *and no unfavorable condition of the weather is to prevent this movement being carried out.*

"5. The force will be broken up into as many detachments as the number of the medical officers will admit, allowing *one to each party.* Should the medical staff be insufficient to afford such medical aid to the several detachments, experienced medical subordinates will be placed in charge of the smaller or less distant parties.

* * * * * * * * * *

"7. The sick, laboring under other diseases than cholera, will move with the force, and share the benefit of the removal from the choleraic atmosphere."

"8. It must be insisted on, that all discharges from stomach and bowels of cholera patients be instantly removed and buried in pits.

"9. Strong disinfectants are to be thrown into the receiving vessels, as well as into the pits, latrines, and privies.

"10. Should cholera follow the troops, they will be moved short distances, at right angles, if possible, to the prevalent wind and track of the disease, every second or third day, care being taken that the marches in no way fatigue the men.

* * * * * * * * * *

"13. One of the several antidotes is the early treatment of premonitory symptoms, of which looseness of the bowels is a principal one; commanding officers are therefore requested to give the most precise orders on the subject, and to cause all men affected by premonitory symptoms to be placed at once in a premonitory ward.

* *Asiatic Cholera—Its Mode of Spreading and its Prevention.* By Wm. Budd, M.D. Bristol: 1865.

"14. The troops are not to return to cantonments until all cases of the cholera shall have disappeared from the neighborhood, either amongst the European or native population. The barracks and hospitals will be thoroughly fumigated, the walls whitewashed, and the doors and window-frames painted, before they are reoccupied."

* * * * * * * * * * *

This admirable Order has nearly put an end to cholera in the army in India the past three years, notwithstanding the extensive prevalence of the epidemic during nearly the entire period. As illustrating the faithfulness of this surveillance upon the cholera, we are informed that officers are always "kept on duty at the latrines to watch the men," to advise them, and to report immediately to the medical officer when any soldier has a diarrhœa.*

This care of a vast army is practically illustrated, *mutatis mutandis*, in the house-to-house visitation which the Academy has recommended when cholera appears in cities. But this special care of individuals and families must always be accompanied by the effectual abatement of all sources of foul air. Upon this point the facts are well presented in the preceding reports upon Medical Relief, Disinfection, and Ventilation.

* Prof. Parkes states the results of his study of preventive measures against cholera as follows: "But as far as preventive measures are concerned, the researches of the last few years seem to have given us indications on which we are bound to act, though they are based only on a partial knowledge of the laws of spread of this poison. Thus it appears that the carriage of the disease by human intercourse, always strongly advocated by some writers, is now quite certain, and it seems also pretty clear that the singular erratic spread of cholera can be best accounted for in this way. This does not exclude other modes of transmission (which, indeed, are highly probable), but it gives us at once a preventive indication of value. The transmission of the disease by the choleraic stools, either when fresh, or more probably when putrefying, is another fact which seems to be now proved, and by tracing out this mode of transmission we can account for many obscure points in the history of cholera. Its evident connexion with imperfect sewerage, and yet its absence from those parts of a town which are worse sewered even than the places it attacks, are best explained as in the analogous case of enteric fever, by believing that the entrance of a specific agent is necessary, but that its further transmission is by means of bad sewerage conservancy. That the stools will dry up, and then in the form of dust may be blown some distance, and may afterwards in some way not yet clear (by respiration? or by being swallowed?), be received into the body, and then act, is also highly probable, though precise evidence is yet wanting." (See Prof. Parkes' *Principles of Hygiene.* Article, Asiatic Cholera.) This most learned and practical hygienist states, in a recent note: "We are bound to take the precaution of adding certain disinfectants to every cholera discharge, and to avoid with greatest care all chance of these discharges putrefying about our houses. I am not speaking too strongly when I say I should consider it criminal in any one to neglect this precaution in the face of evidence which the last few years have given us."

MEDICAL RELIEF.

The sanitary measures against cholera need to be directed to definite objects in every city or town; they should be guided by the most expert knowledge and skill that can be brought into public service. As it is the avowed object of these reports to aid medical men and the public throughout the country to prepare for the epidemic which now seems to be impending, we would present the following conclusions and resolutions as embodying certain propositions which we believe to be important in the councils and measures of medical men and local sanitary authorities.

CONCLUSIONS.

1. That in every city and town which is in any way liable to be visited by cholera, the medical profession should prepare suitable plans for the protection of the inhabitants against this epidemic, and that every local Board of Health should act upon the plans thus prepared, and should also bring into its councils such expert medical men and hygienists as the public welfare may require.

2. That in the removal and prevention of *localizing causes* of disease, and in the universal hygienic surveillance of the classes and localities most liable to cholera, together with the immediate medical care of the sick, the sanitary control of their dwellings and premises by effectual cleansing and disinfection, as advised in this report, two of the most essential factors of a cholera epidemic, viz. the *localizing conditions* of disease, and the *infection from the discharges from the sick*, are at once controlled. And if there are other factors concerned in the propagation and progress of epidemic cholera, as probably there may be, there are sound reasons for believing that they *alone* cannot maintain an epidemic.

3. That inasmuch as single, widely scattered, and popularly doubted cases of Asiatic cholera are, in frequent instances, succeeded by sudden, wide-spread, and virulent local outbreaks of cholera; and inasmuch as in frequent instances the most deadly and extensive epidemics are associated with such small beginnings, it should be deemed a duty of vital importance to the public welfare to have all such *early* or *preliminary* cases of cholera placed under absolute sanitary control, and in every town and city to *be prepared beforehand* to exercise such sanitary care at the earliest appearance of the disease. Hence the Academy of Medicine has recommended the plan of house-to-house visitation, and the sanitary police regulations which are set forth in these reports.

4. That where cases of cholera occur, or are liable to occur, the disinfection and cleansing should be of the most effectual cha-

racter, and that *in the presence of the disease* this duty should not be neglected for an hour; also, that for such disinfection as is required, we cannot trust to mere *deodorants*, but should employ the most speedy, effectual, and permanent *antiseptics;* and moreover, that however thorough the disinfection, it should be regarded simply as collateral to *sanitary cleansing, drying, and ventilation.*

5. That the sanitary control and suppression of Asiatic cholera, wherever its outbreaks occur, is a duty that is of momentous importance to the welfare of the entire country; and that physicians and sanitary authorities should promptly act upon all practical questions relating to this duty without waiting the adjustment of theories, remembering that it is the first duty to prevent or control the earliest outbreaks of this disease.

6. That the most prompt and *rational* action of sanitary authorities may, and in all cases should, be so conducted in the management of the duties here set forth, as not to cause any panic or popular anxiety; and that, properly conducted, these works of sanitary prevention will be the means of preventing alarm, and of promoting public or personal tranquillity.

7. That medical practitioners who witness Asiatic cholera, should, in all cases, preserve accurate notes of each outbreak, each case, and every step in the progress of the epidemic; and, that the hygienic lessons which cholera teaches should be faithfully observed and recorded.

PREAMBLE AND RESOLUTIONS.

Whereas, The New York Academy of Medicine has endeavored to promote among its own members, and throughout the medical profession, a spirit of exact and practical inquiry into the preventive and remedial treatment of epidemic cholera; therefore, be it

Resolved, That this Academy hereby expresses its confidence in the utility of general and specific hygienic measures as the best means of protection against the pestilential prevalence of cholera in any locality where it makes its appearance; and that the most thorough scavenging, cleansing, and disinfection are absolutely necessary means of averting this pestilence in the cities and populous towns of our country at the present time.

Resolved, That in the judgment of the Academy, the medical profession throughout this country should, for all practical purposes, act and advise in accordance with the hypothesis (or the fact) that the cholera diarrhœa and "rice-water discharges" of cholera patients are capable, in connexion with well-known localizing conditions, of propagating the cholera poison; and that rigidly enforced precautions should be taken in every case of cholera to permanently disinfect or destroy those ejected fluids by means of active chemical agents; also

that with the same object in view, the strictest cleanliness of person and premises should be enforced upon all who have charge of the sick; also, that all privies, water-closets, and cesspools should be kept thoroughly under the control of disinfectants.

Resolved, That we regard the nature and causes of cholera infection, so far as the sick or their discharges can propagate it, as being so susceptible of control that there should be no fear or hesitancy in the personal care of the sick and all that pertains to them.

Resolved, That immediate and thorough cleansing and disinfection of all persons, clothing, and things that have been exposed to the discharges or persons of the sick with cholera, constitute the chief end and object of any rational quarantine or external sanitary police regulations against cholera.

Resolved, That, for the purposes here mentioned, an external sanitary police is desirable in all great maritime and river towns, but that such sanitary regulations need not seriously embarrass commercial intercourse and the interests of trade.

Resolved, That the main source of protection against epidemic cholera at the present time is to be found in the vigilant and effective operation of sanitary measures, municipal, domestic, and personal.

Resolved, That the New York Academy of Medicine cordially invites the physicians of every city and village throughout our country to urge the immediate adoption of adequate measures of sanitary protection against the introduction and ravages of cholera, and that to this end we pledge our brethren and the public the hearty and continued coöperation of this Academy.

STATED MEETING, JUNE 20, 1866. DR. JAMES ANDERSON, PRESIDENT, IN THE CHAIR.

The following gentlemen were elected Resident Fellows: Drs. R. F. Weir, E. B. Dalton, C. R. McClennan, Oscar G. Smith, and Benjamin Howard.

LAMINARIA DIGITATA—SEA-TANGLE.

DR. JOSEPH WORSTER exhibited a specimen of the Laminaria Digitata (Sea-Tangle) which he had procured from the coast of the Bay of Fundy. He remarked that the ordinary specimens of sea-tangle heretofore in use were open to the objections of being brittle, slimy when moistened, and offensive in odor. All these objections were done away with in the specimens presented; they were very elastic, would on immersion in water swell to eight or ten times their natural diameter, and could be retained within the uterine cavity for several months without producing any fetor

whatever. Not being slimy, it did not have the tendency to slip out, as did those other specimens of the weed taken from the coasts of the straits of Magellan and the English Channel. Messrs. Tiemann & Co. had a large quantity on hand, and were fashioning it in such shapes as would meet the wants of obstetricians and surgeons.

The resolutions appended to the Report of Dr. Harris on "Police Sanitary Measures," etc., were then discussed and unanimously adopted.

PORTABLE HOT-AIR BATH.

Mr. Ronzetti exhibited his portable hot-air bath. It consists of a simple tin case or box, perforated longitudinally to admit of a pipe of sheet-iron, horizontally placed, a curved joint to be attached to the pipe outside, and an alcohol lamp to supply the heat. The apparatus was then referred to the appropriate Committee for examination.

The Academy then adjourned.

Informal Meeting, July 5, 1866. Dr. James Anderson, President, in the Chair.

There being no quorum present, and it being the last assembling of the members together previous to the recess, the President suggested the propriety of holding an informal meeting.

Dr. Bell, of Brooklyn, having been appointed to read a paper on Disinfection by Steam, stated that he had not prepared one, as he had chosen to wait the results of some interesting experiments then pending. He, however, ventured to give a report of progress. In this report he stated that a paper advocating the use of steam as a disinfectant was first published by him in the Transactions of the Medical Society of the State of New York for 1844. In that paper he, however, only expressed the opinion that steam was a valuable and effectual disinfectant; but since then he has had opportunities for reducing his theories to practice. At a recent trial at Seguine's Point, it had been proved that there was a possibility of applying steam to a degree sufficient to destroy organic bodies. In this connexion he detailed his experiments in disinfecting ships while connected with the Navy, and the good effect they had in arresting the spread of Yellow Fever. He contended that steam was *the* disinfectant *par excellence*, and could be uniformly relied upon in producing the desired result of destroying all fomites, without injury to clothing or to furniture. The heat of the steam was sufficiently high to re-evaporate the vapor in an apartment and render it perfectly dry. He then referred somewhat in detail to the experiments

with the superheated steam at Seguine's Point, and promised at a future meeting to give a full report of several trials yet to be made.

The PRESIDENT then exhibited Rankin's Disinfecting Chamber Pail, and invited the members present to remark upon it.

DR. GRISCOM said that the idea was a splendid one, inasmuch as not only could the stools be disinfected, but when the lid was applied to the water-closet the various sewer gases could at the same time be destroyed.

DR. HAMILTON suggested that the cover would not be applicable to anything else besides dry powders, and these, he thought, were objectionable in this, that when the water-closet was used there was generally more or less of splashing afterwards. He thought that it would be a good idea to have an apparatus so arranged, that it could not only contain fluids, but should be self-acting.

The PRESIDENT said that this could easily be done, by having the pipe containing the disinfecting fluid connected with the pipe which lets the water into the basin.

DR. HARRIS remarked that it was very necessary to have some self-acting apparatus connected with every commode, but more especially with every public water-closet, as very few would care to turn a knob or pull up a valve. This would be especially the case with the public water-closets at railroad stations, saloons, etc. If such a contrivance had been connected with the privies at Elizabethport, it was evident to his mind that the epidemic of cholera which had been reported there would probably not have extended. He then proceeded to give a history of the epidemic at that place.

A short distance from Elizabeth City is Elizabethport. The Germans from the latter place are in the constant habit of wandering up through Elizabeth City, some to take transportation by railway, and others to tarry in the city. About three-quarters of a mile from Elizabethport there is a street (Schiller) occupied almost entirely by Germans; only one rookery being occupied by the Irish. In that street a number of German families tarried, some of whom had been on board of cholera ships. These facts were stated to their friends. About five days after a man with diarrhœa had left with his family, the people in the neighborhood began to suffer from cholera. The first case of a man with a large family was traced to the patient who had diarrhœa as the cause, the former having been, it is said, in the habit of using the same privy as was used by the latter. This first case was attended by a devoted daughter, and the soiled clothes were some of them left lying about the floor, while the dejections were carried to the privy, without, however, being disinfected. The little girl was attacked the morning after the father's death. The father survived eighteen hours from the first onset of the disease,

the girl only nine hours. Seven other cases died in the same house, all being in the habit of using the same privy.

The Mayor of Elizabeth hearing of this, determined to stay the progress of the epidemic by destroying the albuminous fluids wherever found. This he did at a cost of $3000, which was afterwards refunded to him by the City Council, who approved of the means adopted. The first case occurred on Monday morning, and the last terminated by Saturday night. Notwithstanding that the inhabitants were in a fit condition to invite the epidemic, and were, in fact, only waiting for its fearful ravages, without attempting to guard against it, yet by the simple means adopted by the Mayor, who, by the way, is a medical man, the epidemic ceased at once.

In answer to a question from a member, Dr. Harris stated that one of the best fluid disinfectants was the ordinary coal-tar residue.

Dr. Hamilton suggested that a Committee be appointed to devise some improvement of the apparatus of Rankin, in accordance with the suggestion made.

Dr. Griscom thought that only such a contrivance might be made as would render the apparatus more effectual for the water-closet; as it was, he thought, the chamber-pail answered every purpose.

Dr. Richards remarked that some of the disinfectant might be dissolved in the tank above the water-closet, and might by that means find its way into the basin below.

After some further discussion, a Committee consisting of Drs. Griscom, Post, and Harris, was appointed to report upon the expediency of making improvements in the application of this apparatus to the various purposes alluded to.

The Academy then adjourned for the summer recess.

Stated Meeting, September 19, 1866. Dr. James Anderson, President, in the Chair.

Dr. Pomeroy read a paper on Inflammation of the Cavity of the Tympanum, and its relation to the *Exanthemata*, of which the following is an abstract:—

The object of the paper was to exhibit the frequency of aural inflammations dependent on an attack of some one of the exanthemata, more especially measles and scarlatina, and to give a rough sketch of the pathology, diagnosis, and treatment of such cases.

The inflammations of the middle ear, whether or not dependent on an exanthem, were regarded as commencing in the throat, as a rule; a small number of cases, however, arose from the exanthematous eruptions involving the ear externally.

An inflammation of the throat in an exanthem was described as being treated if severe; but if not, it was neglected, to the life-long detriment of the organ of hearing in many instances.

In the records of hospital and infirmary practice, a very large number of old inflammations of the middle ear are traceable to a former attack of an exanthem. Troltsch was here quoted to this effect, viz. evidences of this disease were found on the dead body in children, and traceable directly, to a previous exanthem. The general symptoms in an exanthematous fever seemed to overshadow, from their exceeding gravity, the aural symptoms, and consequently were neglected, which Troltsch regarded as highly reprehensible.

Wilde was also quoted; the substance of which is as follows, viz. out of two hundred successive applicants at the hospital for ear diseases, on which he was in attendance, twenty-eight were found to be directly connected with the exanthemata. He also speaks in the same vein with Troltsch—that physicians neglect attending to the aural complications in the exanthemata, which are amenable to treatment, and neglect of which leads to very serious consequences. He quoted certain foreign tables to show that out of one thousand eight hundred and ninety-two cases of acquired mutism, two hundred and sixteen were from scarlatina, eighty from measles, and twenty-eight from small-pox; and from American tables, out of eighty-six cases of non-congenital mutism, forty-one were from scarlatina.

While engaged under the Census Commission in Ireland, he found in three hundred and seventy-four cases of acquired mutism, that thirty-five were from scarlatina, twelve from small-pox, and seven from measles, and expresses a belief that this proportion is not as large as actually existed. His most unmanageable cases of otorrhœa have been the result of scarlatina or measles. Kramer, Leincke, Pilcher, and Harvey of London, were alluded to as entertaining similar opinions.

The inflammatory attack generally commencing in the throat, passes up the Eustachian tube to the middle ear; if it is mild in character, it soon subsides, doing little harm; but if the disease is violent, we have an excessive thickening of the mucous lining of the part, which very often closes the Eustachian tube, and which, if long maintained, results in the falling inwards of the membranum tympani. As the air in the cavity is absorbed, permitting of pressure only upon the exterior of the membrane, this, of course, prevents it from vibrating freely. Quite frequently, however, the membranum is ruptured, and the resultant otorrhœa becomes a very annoying complication. The ossicula may be anchylosed as a result of the inflammation, so as to hinder their vibration, and thus injure the hearing power. The canals leading to the mastoid cells are sometimes closed, resulting in the production of a smaller vibratory chamber than is consistent with the integrity of the organ.

The extension of inflammation into the complicated structure of the internal ear impairs its function, by disturbing the delicate equilibrium upon which its integrity depends; this accident very

often results in injury to the auditory nerve, which is undoubtedly a very common cause of deafness, as disease of the optic nerve is of blindness; like its congener, the optic nerve, it is subject to atrophy, to infiltrative products of inflammation, and other degenerations.

The signs of inflammation of the tympanic cavity are, a sunken membrane, as indicated by undue prominence of the handle of the maleus, alteration of the light point, redness along the line of the handle of the maleus, or a general redness; and in slight cases, a dulness of the normal polish of the membrane, producing an opacity in some instances resembling ground glass. In very old cases, besides this, we may find lines and ridges of fibrous tissue traversing the membrane, or presenting a greyish white calcareous degeneration; occlusion of the Eustachian tubes, considerable deafness, a feeling of fulness in the ear, the severe lancinating pain, sore throat, soreness about the auricle when pulled or moved, pain in deglutition or mastication, or during coughing and sneezing, etc.

There may be considerable difficulty in detecting the disease in infants affected with a grave attack of exanthema, who, from the frequency of such affections in children, rather than in adults, will be the kind of patients presented to us. The child cries, evidently from pain; it tosses its head about; it starts in sudden fits of crying, or screams outright as though suffering the acutest pain—symptoms common to other than ear diseases; if you examine the membranum tympani, you do not necessarily find signs of inflammation; there may be tenderness about the auricle; the throat usually is inflamed, but that does not prove inflammation of the ear. In an exanthem, we do not, unless there are complications, often have such exhibitions of pain; and if we have this symptom and others pointing to aural trouble, we had better treat the patient as though we were certain of our diagnosis.

It may be remarked that ear affections consequent on the exanthemata have been observed to run a short and comparatively harmless course, quite unlike the picture here presented. This, in a few instances, is true enough; but it cannot be always determined whether the affection is to result in a grave prognosis or not; and we do well to always look carefully to its treatment, for fear that neglect will lead to bad results.

Treatment.—The indications undoubtedly are, to subdue by proper antiphlogistics the *inflammation*—which is the sum-total of the disease. The pain, which is so characteristic a symptom, may be relieved by pouring a few drops of Mag. sol. morph. into the external canal; dry warmth, as a heated plate enveloped in a napkin, and applied to the auricle; pouring warm water into the meatus; poultices, except they should not be long continued, for fear of inducing otorrhœa by their macerating effect upon the part; and one or more leeches may be applied to the external meatus. A roasted onion, as it retains the heat well, is a very

good agent; its stimulating properties are, it seems to me, hardly injurious; and it appeals to the sympathies of a certain class of patients, who would have no confidence in a simple agent like warm water.

The object of injecting air into the cavity is more than simply to render the Eustachian tube pervious: the membrane is often fallen inwards, that is, sunken; and it is restored only by inflation of the cavity with air, or gas, which simply acts mechanically, by pressing the membrane outwards. Any adhesions forming between the membrane and inner wall of the cavity, may sometimes be ruptured by the forcible pressing out of the membrane, during inflation. Secretion may also be removed from the cavity by this plan, either through a perforate membrane, or by the Eustachian tube. This inflation should be continued daily, until the indications for so doing no longer exist. In using medicated vapors or gases, Politzer's plan, with a Buttle's inhaler adapted to the nostril, is the most convenient mode. A Buttle's inhaler, devised by a medical gentleman of this city, consists of a hollow cone, in which a sponge is placed, being first moistened by the medicament, and opened at either extremity, to admit of the free passage of air through it, which becomes more or less laden with the medicament. Tincture of iodine I have usually used in this apparatus; although chloroform, ether, aquæ ammonia, ol. terebinth., tincture of camphor, etc., may be used. The combining the Buttle's inhaler with the Politzer plan is due to Dr. Roosa, of this city.

If the case cannot be managed, except by the catheter, a flask must be adjusted to it, from which the gas or vapor is formed. The muriate of ammonia, heated to about 212° Fahrenheit, is usually preferred; which is forced into the ear by an air pump, or rubber bag attached to the flask. If the catheter alone fails to render pervious the tube, a bougie may be passed through the catheter into it, so as to dilate any stricture which may be present, and break up adhesions within the cavity. This is, however, very difficult, and in many instances hazardous, as the ossicula have been disarticulated by the passage of the bougie into the cavity. Moreover, it is a very difficult manœuvre, and should only be attempted by a skilled hand.

I hardly think it proper to make any attempt towards rendering the tube pervious, however, until the acute symptoms have somewhat subsided, as pain and aggravation of the trouble are many times occasioned by adding by inflation to the intra-aural pressure already existing; and it is comparatively harmless for the tube to remain impervious a limited period; and nature is prone to open it by means of coughing, sneezing, etc. The object of the aeriform medicament is, of course, to act on the inflamed mucous membrane of the diseased part. I do not know as any constitutional treatment is necessary, beyond attending to the secretions, allaying any undue febrile movement, etc. Mercury,

used as an alterative, I am inclined to believe to be comparatively useless in any stage of the disease. Whenever the membrane is ruptured, a persistent otorrhœa is generally the consequence; and is best treated by two-grain solution of acet. plumb., or tannic acid dissolved in glycerine, at the rate of from ten to sixty grains to the ounce; or alum, sulphate of zinc, and other astringents of this class. Very often we observe a granulated condition of the lining of the cavity, which may be touched by a solution of arg. nit.; or the same, fused upon a probe. Larger granulations may be cautiously touched by nitric acid. In old cases, which have resisted treatment, stuffing the cavity with cotton wool has been quite successfully practised, on perhaps a similar theory to the curing of indolent ulcers of the leg by strapping. It is important to first dry the ear with the cotton wool, and then introduce it very carefully, but at the same time pressing it firmly into the ear, taking care not to fill the external canal, as pain may be occasioned. This should be removed, and reapplied every one, two, or three days. The plan is to be further tested; but at present it seems to do, in many cases, more good than any other mode of treatment.

Dr. Roosa agreed, in the main, with the views just expressed, but thought that Dr. Pomeroy had somewhat exaggerated the difficulty of making a proper differential diagnosis in the case of the middle and internal ears. The condition of the membrana tympani he regarded as a sufficient guide. For the examination of this organ, in young children, all that was absolutely essential was a broken bit of an ordinary mirror, from which just enough of the silvering had been scraped for the convenience of the observer. The next step would be a stretching of the auricle, in such a manner, of course, as not to alarm the little patient, with a view to straighten the natural curve of the canal. He was aware that the inflation of the cavity, during acute inflammation, by the Valsalvian method, caused great pain; but he would not, on that account, entirely abandon it. Since it was very important that in the child the Eustachian tube should be kept open, the physician should address himself to calm its fears, and resort to the well known method of Politzer. Ordinarily, however, the patient is not frightened; and we have the satisfaction of knowing that the two little muscles concerned in deglutition may be induced to perform their work more readily, after having been once or twice assisted.

He would allude to an improvement in the Politzer apparatus, of which he had lately been made cognizant, and of which Dr. Weber was the first adapter. This, by the addition of Dr. Clark's nebulizer, converts the second rubber bag into a reservoir, and so maintains a constant, instead of an interrupted current. Dr. Weber also employs a little receptacle for vapors, beyond this second bag.

He would, in this connexion, comment in a general way upon the impression particularly current among country practitioners,

that the whole subject of aural diseases was very intricate, and required the undivided attention of the specialist. Nothing was wider of the truth, and nothing was more productive of serious error. Dr. Pomeroy himself was a practical refutation of this misconception, since, notwithstanding the cares of a general practice, he still found time to devote to the study of these diseases.

Dr. N. W. J. Heath then read a paper entitled:

ASIATIC CHOLERA, AS IT APPEARED ON THE STEAMSHIP "ENGLAND," IN APRIL, 1866.

The steamship "England" left Liverpool on the afternoon of Wednesday, March 28, 1866, with eight hundred passengers, principally German, some English and Irish, and a mixed crew of one hundred and thirty seamen, making a total of nine hundred and thirty souls. The day of leaving was wet and disagreeable; and many of the passengers were soaked, as also their bedding and luggage. Their bedding, I may mention, were large feather ticks, principally belonging to the Germans. The Germans were placed in different parts of the steerage, between decks—not confined to any one place, but mixed promiscuously with the other passengers; but the steerage under the saloon, or the "after steerage," as it is called, was most thickly crowded.

After about twenty-four hours' steaming, we reached Queenstown, and took on board about four hundred more passengers, who were similarly distributed throughout the ship; these were all Irish. We left Queenstown then about 5 P.M., March 29th, with our living freight of about thirteen hundred and thirty souls on board. It soon began to blow hard, and in a day or so all the passengers were kept down by the severity of the weather, which lasted for two days, the ship taking plenty of broken water over her; and, consequently, the steerage and orlop decks were wet, dirty, and of course unwholesome, from which a most peculiar and disagreeable odor emanated.

They could not be sufficiently well cleaned in consequence of the space the passengers occupied when "all down below." Chloride of lime and disinfecting fluid were used with beneficial results. To add, as it were, to the general unhealthy condition of the decks below, females, and even some men, emptied their tins of excrement and urine behind boxes, etc., and wherever they could conceal it. All this time the people had to sleep on their damp beds, not having had an opportunity of drying them; but notwithstanding all this hardship they were, to all appearance, well, healthy, and in good spirits. They were fed three times daily on good, substantial, and wholesome diet.

Breakfast.—Consisting generally of fresh bread and butter, with either coffee or tea, and sugar.

Dinner.—Soup, vegetable or pea; fresh meat, roast or boiled;

potatoes; and on Thursdays and Sundays, plum-pudding was added.

Supper.—Tea, sugar, and biscuit.

On the fifth day after leaving Liverpool I was sent for, in conjunction with my colleague, Dr. McCullough, to see a man reported by the steward on watch as "suddenly and dangerously taken ill." We visited him, and found, to our surprise and regret, that he had all the symptoms of Asiatic cholera. He lay on the top bed, with his head slightly raised, motionless, except when cramps seemed to annoy him, his whole countenance shrunken and of a dark leaden hue, covered with a cold perspiration, which, in conjunction with his shrunken features, deep, lustreless eyes, contracted lip, betraying the dry and parched mouth, from which the cold breath could be distinctly felt even at a distance, tended to strengthen our diagnosis and stamp the disease. He involuntarily passed rice-water stools, as they are called, but no urine, and constant vomiting served to annoy him; he spoke in a peculiar voice, I should rather say whisper, belonging to the disease—it is capable of being heard at a long distance, even six to eight feet, and always spoken with considerable clearness. His nails were purple-black; his hands shrunken and clammy; he was pulseless at the wrist, and the action of his heart was weak. It only took three hours for this terrible disease to work such a dissolution in a strong, healthy, temperate individual. We treated him with stimulants; brandy and beef-tea in small and oft-repeated doses; laudanum, in thirty-drop doses, every thirty minutes; mustard sinapisms to the epigastrium, and hot water bottles to his extremities; but of no avail, the choleraic influence had too strong a hold of him. Notwithstanding this treatment, as might be expected, he died in twelve hours after we first saw him. He was immediately sewn up in canvas and buried. The next case, similar in all its details, occurred in the other end of the ship, which we treated with fifteen grains of calomel and two grains of opium. I should also mention, in connexion with this case, that there was little, if any, intercommunication between the two steerages. This treatment, also, in this case, failed: although in many cases, when combined with the solid and fluid preparations of camphor and ammonia, it happily succeeded. And while speaking of camphor, I must say, and confidently so, that it has acted in our hands most successfully, in five to ten-grain doses, before the excessive vomiting commences. It seems to possess a peculiar, successful control over cholera. We gave it generally in combination with moderate doses of opium, either solid or fluid.

I have seen in many cases of cholera, strong bilious discharges brought about by the use of calomel, and syncope ensuing, carry off the sufferer. Might not that have been consequent from exposure and want of proper nutriment and attendance, evils constantly attendant upon a sea-voyage? After this, cases occurred promiscuously throughout the ship, sparing none in its

deadly course, except cabin passengers, officers, and engineers, spreading amongst the firemen, sailors, and passengers.

One sailor died, consequent upon the exhaustion supervening the reaction; but a good bilious discharge, by the frequent use of calomel, had been previously set up for some days before he died. Stimulants of every kind, with light nutritious diet, were administered, and opium, to procure rest, but notwithstanding all, he sank and died after six days' illness.

Amongst the firemen, the disease made more havoc than the sailors. From their extremely unhealthy way of living, their constitution is more liable to infection, and with them the disease was more fatal. We had five deaths amongst them from eight cases of Asiatic cholera. All were treated by the calomel and opium treatment, and only in three recoveries were the true bilious discharges set up.

Strange to say, no diarrhœa preceded the attack of cholera upon the ship; but during the prevalence of it, and long after it left the ship, strong diarrhœa afflicted all. This, I think, is a most important fact as regards the theory of cholera being propagated through the air.

And here I might mention that the steamship "Virginia" took cholera seven days after us, in the same latitude and longitude nearly as we did. A week succeeding that event, the "Erin" was attacked with violent diarrhœa, in the same place, as I have been informed by Dr. Chapman, of the "Virginia," and Dr. Smith, of the "Erin."

That is a remarkable incident. But to resume: A question of doubt arose; should we proceed or not? We decided upon going to Halifax for the following reason: when the cholera did really assume its sway, we were more than half-way across the Atlantic, between Halifax and Queenstown; and as we were bound to a western port (New York), we deemed it would not be either prudent or proper to pass a port in our western progress—so, consequently, we shaped our course for Halifax, Nova Scotia; and accordingly, on the morning of Monday, May 9th, 1866, we entered Halifax and anchored off an island called McNabb, in the Quarantine ground. I may here mention a remarkable, a strangely curious incident, and have done with it: The Halifax pilot did not come on board, but was towed up in a boat astern; he passed his license on board in a bucket, for Captain Grace to see, and it was returned to him; he piloted us up to our destination, rowed ashore, and he, and two or three more in the same place, were dead in a few days, from what was reported Asiatic cholera: however, there it stopped. We had now lost about sixty-three people.

The late Dr. Stayton, health officer of Halifax; Dr. Gossip, present health officer; and Drs. Gorrey, senior and junior, came off voluntarily to assist us in our arduous duties. We transferred our well passengers to McNabb's Island, and our sick we placed on a

hulk called the Pyramus, sent down to the Quarantine for our use for the sick. Without the slightest accommodation—no beds or bedding, neither food, medicament, nor anything necessary for well people, much less for sick passengers, into this floating house of emptiness we put about one hundred and fifty sick people—some with only slight symptoms, some afflicted with ship fever in its various forms, and others in the different stages of that dire disease, Asiatic cholera; there they had no accommodation but the hard deck to lie upon; and until we had time to bring them our own beds, also bedding, food, medicine—sent from the "England" in small-boats—they had nothing. This took some hours; and now, after a day or so, strange to say, the disease as it were appeared to change its character, and ship fever in its worst form, in its most fatal character, assailed us in its place, fully as bad as the cholera in its rapidity of action, and in its certainty almost of producing a fatal result. One case in particular I especially recollect, and now relate: The case, a fine, healthy young girl, about twenty-two years of age, came off to the hulk to attend her sister, in Asiatic cholera, and who subsequently died. The day after she died this girl complained of a sensation of lassitude, with headache, accompanied by vomiting to a great extent, and slight purging; but no cramps, nor had the evacuations the strange and white appearance of choleraic stools; there was no suppression of urine; her pulse was weak and thready; her tongue covered with fur, and her eyes glazed and expressionless; a severe headache served to prevent her rest, and insufferable thirst continually annoyed her. I ordered her diaphoretics: light chicken-broth and wine; mustard over the epigastrium. But, two or three days after the occurrence of the above, I left in the "England" for New York, and when, about three weeks or a month afterwards, I visited Halifax, in the steamship "Louisa Moore," she had completely recovered under Dr. Gorrey's treatment—the principal portion of which was light nutriment, stimulants, and saline draughts. Another case I shall just cite, that of a young girl who took cholera on board the day before we arrived in Halifax. We found her on deck, nearly altogether collapsed, with all the symptoms of the first case I quoted. We commenced with two-grain doses of camphor, brandy and laudanum; small quantities of beef tea; mustard sinapisms to the epigastrium, etc., and warmth to the extremities. These applications and this treatment, steadily persevered in and vigorously carried out, afforded us the gratification of observing the heat return to her extremities, the pulse gradually to assume its wonted frequency, the vomiting to cease, and the character of the fæcal discharges, *without any aperient medicines*, assume a more healthy form, and lessen in quantity; the cramps left, and she felt inclined to sleep. After resting for about three hours, she awoke so feeble that she was hardly able to raise her head. As I said before, we arrived at Halifax next day. She was placed in the miserably fitted up hulk, and in six days

after admission, notwithstanding all the attention which was bestowed upon her, she succumbed to the virulence of ship fever. Many more cases I could cite of a similar character, but as no additional information could be gleaned from them, I shall not record them. As soon as the anchor was down, and the ship stopped moving through the water, the ventilation was considerably lessened in the steerage, the result of which was an immediate increase of the mortality. I am not sure now how many died the night we arrived in Halifax, but certainly twice as many as in any one night at sea, clearly showing the great necessity for ventilation in the treatment, prophylactic and otherwise, in contending with this terrible disease. All our well passengers, with the exception of a few delicate ones who escaped our vigilance, were placed on McNabb's Island, and one day after their arrival there, notwithstanding their privations, which were many, not a single case of real cholera occurred; they suffered a good deal from severe diarrhœa, ship fever, and exhaustion, coupled with the effects of cold—but no cholera. Many of them died from the above causes, and amongst the rest, one whom many have regretted and will scarce cease to mourn the loss of; I speak of Dr. Stayton, of Halifax, health officer. His symptoms, those of cholera-morbus, did not long delay a fatal termination. He resisted alike the effects of stimulants and medicaments.

Nearly all the passengers in the hulk died; and when we left Halifax, May 18th, very few indeed were left on board of her alive. On the Island, diarrhœa for four or five days caused the passengers considerable annoyance, but was easily controlled by the usual astringents, chalk mixture, sugar of lead, opium, etc. A few cases of low typhoid fever occurred, but under the usual routine treatment many of them recovered. A few deaths did occur on the Island, but only the old and infirm, the very young and feeble, and those who either could not or would not apply for medical aid. Exposure, cold, and dampness, hurried many cases to an unavoidable fatal termination. During all this time, from May 9th to 18th inclusive, the steamship "England," in quarantine, of course, underwent a most rigid cleansing and fumigation, thoroughly carried out under the immediate superintendence of one of the ship's officers, and under the control of one of us surgeons. On the 18th of May the health officer, having considered the matter, determined to permit the healthy passengers to come on board, and, with a clean bill of health, to proceed to New York. We accordingly passed on board the healthy passengers—I should think about eight hundred and fifty—and at 5 P. M. proceeded to New York. We reached this port on Saturday, May 21st, 1866, at Lower Quarantine, where Dr. Bissell boarded us. We had no sickness from Halifax, and, strange to be observed, notwithstanding the diarrhœa which prevailed on McNabb's Island, in Halifax, not a single severe case occurred amongst us during our passage from

that port. We left on McNabb's Island some fifty or sixty; some suffering from pneumonia, more convalescent from ship fever, and severe diarrhœa. The "England" was detained at the Lower Quarantine twenty-two days, with all the passengers on board; but so thoroughly had the disease been eradicated from the ship, and so well and surely did the change of air, condition, and situation benefit the general health of the passengers, that no disease attacked them save the ills incident to so large a number of people in a single vessel.

Dr. Griscom inquired of Dr. Heath as to the truth of the statement that some of the cholera patients were in the habit of drinking from the same vessels into which they discharged their urine.

Dr. Heath was not aware of any such thing having occurred; was positive that it was not customary; but there might have been a single instance of that kind.

Dr. Griscom contended that the cholera was not introduced upon the ship from the shore, but was generated upon the ship itself. Excessive filth, personal and general; absence of ventilation, and other circumstances, showed that foul miasma was absorbed into the system by respiration, by local causes generating disease. He was satisfied that Dr. Heath's Report demonstrated the local cause of what is called cholera, and that the cholera in this city was not an *epidemic*, but an *endemic* disease, produced by local causes, combined with the excessive hot season.

Dr. Herzog did not doubt that the cholera was in England, or that it was in Holland, when the "England" left; and thought that it must have been brought on board by some of the passengers. In reference to the propagation of the disease on one end of the ship, there existed no doubt in his mind that there were two local foci in the ship. It seemed strange, also, that the pilot, not having gone on board the ship, should have contracted the disease, and have died; his communication being—as he learned from interrogating Dr. Heath—by a bucket lowered over the side of the vessel. He inquired of Dr. Heath if there had been any communication with the shore.

Dr. Heath replied in the negative; and briefly explained the matter, stating that there had not been a single case of cholera in Halifax before the "England" arrived off that place. He thought that there was some connecting link in regard to the occurrence of this case of cholera which had, as yet, not been accounted for.

Dr. Elliot understood Dr. Griscom to ask if the passengers urinated in the same dishes from which they ate and drank; stated, to his certain knowledge, this was done on board a Liverpool ship on which he had occasion to be sailing, several years ago; and was told by the captain of the vessel that it was the usual custom. He then moved a vote of thanks be tendered

to Dr. Heath for the reading of his interesting paper, which was seconded by Dr. Harris.

Dr. Harris remarked as follows:—Mr. President, it is not my intention to occupy the attention of the Academy with any disputation of the points raised by my friend Dr. Griscom. Every member of the Academy joins in this expression of thanks for the truly professional spirit in which Dr. Heath pursued his observations and treatment of the epidemic upon that unfortunate ship.

It is not necessary, in this Academy, to debate the single question that was raised by the speaker that first succeeded Dr. Heath's reading this evening; but, inasmuch as the fact will now go on record in our minutes that a respected Fellow of this body asserts that the cholera, which Dr. Heath has so fully described, did actually and solely originate in the ship "England," and that it was not of exotic origin; that the cholera in New York and in our American cities this year is also self-originated and indigenous—wholly dependent upon local productive causes; and particularly that the "England's" emigrant company did, by neglect of ventilation and cleanliness, produce the infection and epidemic that decimated that ship,—I cannot forego the duty of placing on the same record of our discussion this evening the following facts, namely:

At the same time that the "England" was receiving her passengers at Liverpool, and during the very period of her passage to America, other emigrant ships, at Liverpool and other European ports, were shipping and transporting to New York large companies from the various parts of Europe in which cholera was not prevailing; and several of the latter vessels were far more crowded, far more filthy, far worse fed, and not as well policed on the voyage. The "England's" German passengers came *via* Rotterdam and Hull, from certain towns and districts in Holland and northern Germany in which cholera was, at the time, epidemic. The particulars of each individual's history, among Dr. Heath's patients, do not yet enable us to know if the first cases occurred in persons from one of the infected towns. And this particularity is not essential at this moment; for we know that bedding, clothing, and various things that are soiled by personal excretions, are always to be found in an emigrant ship's baggage when received. We know that the cholera appeared and spread as a pestilence on the "England." It did not appear on ships that brought no passengers from Rotterdam and Holland. But it did appear on the very next ship of the same line, and in emigrants from the same infected districts.

The ships "England" and "Virginia" have, this evening, been charged with the origination of the pestilence that raged in them. I know the ships and the fine steamers of that line; they are among the very best regulated ships that enter our port; and are far superior to the average of ships that arrived at the same period crowded and filthy, and without cholera. And mark this fact,

that as soon as this line of steamers ceased conveying emigrants from infected regions—as they were ordered to cease doing—they too arrived in our port without cholera.

There was such infectious, pestilential character in the malady that was stealthily traversing the Low countries, Rotterdam, Saxony, and Baden, that the Privy Council of Great Britain did, as early as April 23d, issue official instructions to guard the ports of Hull and Bristol, Eng., from that infection; and where the first cases of cholera appeared at Bristol and Liverpool, the sanitary officers treated, published, and guarded them as cases of an exotic pestilence.

In August we had information of cholera on the fine steamer "Bavarian," with only two hundred passengers, coming into Southampton, Eng., after a few hours from the German port from whence they received infected persons. Those sick persons were left at Southampton, and the ship arrived at our port with the pestilence still raging. Then the good ship "Agnes," with a company of healthy-looking passengers, was about sailing from Antwerp. The passengers were yet in the fresh open air, on shore or on deck; cholera suddenly appeared in the ship's company, and the order goes forth from the local Sanitary Government to open the spacious fort in the harbor, and place these people there.

In spite of fresh air and good food, that fine company of emigrants is decimated by cholera in a brief period, and is directly spread through the water-side streets of Antwerp, until more than a thousand perished before September.

In the low streets of New York, during the last days of June and the early part of July, several soldiers are recruited and sent over to Governor's Island, fifteen hundred yards away: other soldiers, from that island, visit the streets where cholera exists, and they return to the Island. At the time when two soldiers are sick with symptoms of cholera, and after others have for days had choleraic diarrhœa, the Government ordered five hundred of those recruits from that island to proceed to the steamship "San Salvador," in our harbor, and sail immediately to Savannah. When only a few hours out from New York, cholera had one of these men in collapse; two others soon followed. There had then been no crowding or uncleanliness on board that could produce cholera.

That company of five hundred men is speedily disembarked on a sandy key, near the mouth of the Savannah. The sea-breezes are fresh and incessant, but the loose sand and vegetable refuse are good absorbents, and they do absorb the choleraic discharges for the first six or eight sick men. After four days cholera is raging as a pestilence in that little camp, and soon it sweeps one hundred and fifty men into the grave. All who could, fled in terror from that camp. The *same* pestilence is soon spread to the negro quarters about Savannah.

Another company of recruits, from the same infected source in

the First Ward of our city, is shipped to New Orleans and Galveston. Cholera goes with them, prevailing on the voyage, and becoming the initial points for the same pestilence at New Orleans and Galveston. The same cause stands associated with cholera at Louisville, Nashville, and Augusta, more recently.

At St. Louis the weather has not been as hot at the beginning of summer, and when cholera appeared, as it was last year when, during my visit in that city, in July, 1865, the mercury stood at 94° day after day in my rooms at the Lindell House. Soldiers and masses of negroes and refugees then crowded that city. I visited their quarters, and the old camping-grounds and barracks about the city, and found them excessively filthy. The war had left the sanitary condition of the streets and quarters very filthy. But *this* year the river steamboats from New Orleans arrived with cases of cholera on board, and with bedding and clothing defiled by the sick. A pestilence creeps along the low quarters of the city, and, after a few weeks, it sweeps almost every square, regardless of wealth and architecture. At its acme of virulence, nearly fifteen hundred were killed in a single week in that fine town.

All these events have occurred as parts of the history of an epidemic. In each place, and under the widest variations, we find still the *same* malady pursuing its course as an infectious pestilence.

Governor's Island and our First Ward, the military transports, Savannah, New Orleans, the infected river-boat companies, Memphis, St. Louis, and a great number of the river towns, have received one of the same kind of pestilential infections.

The resolution was unanimously adopted.

The Academy then adjourned.

STATED MEETING, OCTOBER 3, 1866. DR. JAMES ANDERSON, PRESIDENT, IN THE CHAIR.

CONTRACTED VAGINA.

DR. POST stated that about a year ago he was called to see a young married lady, who was otherwise well formed, but was unable to have sexual intercourse with her husband. He found, on examination, that the vagina was very much contracted, so much so, that the introduction of the finger was attended with severe pain and much difficulty. He proposed treating the case by mechanical dilatation; and, accordingly, put the patient under the influence of ether, and with the thumbs of the two hands forcibly separated the walls of the vagina, and succeeded in effecting dilatation to such a degree that he was enabled to pass four fingers into the vagina. This operation was attended with slight hæmorrhage.

On seeing the patient subsequently, the doctor found that the difficulty had not been entirely relieved; that, in addition, there was some erosion of the mucous membrane, and menstruation was painful and difficult. The treatment was repeated, and the speculum used; the cervix uteri was also dilated with the bougie; after which, an application of nitrate of silver was made. This treatment was continued for several months, when she became pregnant; and was seen by the doctor when four months advanced in pregnancy. The case was interesting in connexion with the communication read by Dr. Sims, as it involved no surgical operation.

A CASE OF URÆMIA.

Dr. Griscom stated that, for the last three weeks, he had had a case under his charge which might be of interest to the Academy—the case of a woman, thirty-nine years of age, who applied to him for relief from some uterine trouble. On investigation, he found a simple enlargement of the uterus (the uterine sound passed in some three or four inches), with vaginal congestion, which he relieved by the use of glycerine.

In a day or two, on investigating the case further, he found that there was an extraordinary suppression of urine; and believed that for two weeks there was not more than two or three ounces of urine voided. The patient did not complain of anything in particular, except some derangement of the nervous system. At the end of a fortnight, coma supervened, accompanied with partial blindness, and finally symptoms of convulsions, such as belong to uræmia; and for the last week there had not been a particle of urine passed from her; and, to his surprise, on introducing the catheter, he drew out about an ounce of pure pus, and nothing else. On Sunday afternoon the patient died. On Monday, the doctor made a post-mortem examination.

A year ago she had had the same difficulty: for a week there was a total suppression of urine; but she recovered, under the care of Dr. Watts.

At the post-mortem examination, the cause of the trouble which had existed a year ago, also as regarded the present time, was, as he believed, discovered. On removing the left kidney, he found it very much enlarged in size, and perfectly disorganized, containing an immense quantity of pus through its texture; calculus very rough in character, and very hard. The right kidney was very difficult to find, there being present an unusually large amount of false membrane, it having been subject to inflammation, the inflammation being the cause of the suppression of urine continuing so long, and to such an extent as to have produced false membrane. The right kidney was found to be in a similar condition to the left—abscesses containing pus, and entirely disorganized.

What surprised the doctor most was the deficiency of the symptoms of a local character; there being no pain—a little tenderness over the abdomen towards the latter part of life, but nothing of any consequence.

Dr. G. said that he mentioned this case more from curiosity than scientific relationship, showing that a patient could live so long without passing any water.

PROTRUSION AND NECROSIS OF A PORTION OF TWO RIBS.

Dr. Finnell related the case of a little boy, about six years of age, who some three years ago was run over by a wagon in the street, was brought home, and his medical attendant pronounced a fracture of the third and fourth ribs. The bandage was put on with more firmness than judgment, and the boy was thus left for four weeks; when, on removing the bandage, the mother found something protruding, and sent for Dr. F. On examination he found two ribs projecting—third and fourth—ulcerated and necrosed. The necrosed portions were clipped, and the boy speedily recovered under simple treatment.

The bandage had been applied with such firmness that the bone had ulcerated its way through, ending in necrosis.

ALBUMINURIA NOT FOLLOWED BY CONVULSIONS.

Dr. Garrish related the case of a lady, twenty-two years of age, whom he attended some three months since. On visiting her he found that she was married—had been married about ten months—and at that time eight months advanced in pregnancy. He found that she was laboring under general dropsy, vertigo, and also very imperfect vision; the eyes could scarcely be seen from anasarca; the extremities were also enormously swollen. On examination of the eyes he found that the pupils were dilated to their fullest extent; and the patient told him that at times she was unable to see anything; that she could not tell one person from another, or shades from colors, and at that time was unable to walk. Some urine was procured for examination, and found to be highly charged with albumen. The gentleman thought at that time that the case must certainly prove to be very unfavorable, and notified the parents that he would have to give them a very unfavorable prognosis of the case; but watched her, and gave her a little medicine; and directed her to take Sal Rochelle, ʒj. On giving her this dose every night it merely produced a laxative effect on the bowels. This was continued for three weeks, when she had three or four evacuations daily. At this time he directed her to take it every third night, which was merely to keep the bowels in a loose condition. At the end of the ninth month she was taken with labor, which was very severe in character. After she had been in labor about an hour she

was taken with the most excruciating pains in the head, and the doctor directed the application of bottles of hot water to the feet, when the pains were relieved; everything went on well, and the bottles were removed. As soon as the pains were relieved, cold water was given. After a while the pain was renewed, and the bottles of hot water were reapplied; the labor-pains increased, and at the expiration of the sixth hour from the time the labor-pains commenced she was delivered of a male child, weighing some ten pounds. After that the patient did very well; the anasarca subsided, and her vision was perfectly restored. She had not one symptom threatening convulsions, except a violent pain in the head. He stated that he had been in the habit of taking blood from the arm, or cupping or leeching the temples when symptoms of convulsions presented themselves; and that it was laid down by Dr. Meigs, wherever inflammation was present, the lady would not escape peritonitis.

Dr. Post had seen several very well-marked cases of œdema of the face, where there were no convulsions; but did not recollect if the vision was affected. He mentioned two cases of anasarca that occurred with impaired vision; one of which had headache during labor, which was relieved by bloodletting; the other had no headache.

Dr. Garrish wished to know the best treatment for such cases, and stated that he used Rochelle salts; that Rochelle salts acted as a laxative, regulating the bowels.

Dr. Griscom had a case of albuminuria of a very decided character, which was cured (if he might so call it) in two or three weeks by the use of diuretics, mild in their character, and diaphoretics: extract of buchu and acetate of potassa. This case was not in pregnancy.

Dr. Bulkley thought that as the urine was not examined under the microscope, the true nature of the case could not be ascertained.

THE BROMIDES OF POTASSIUM AND AMMONIUM IN DELIRIUM TREMENS.

Dr. O. White related a case of a lady suffering from a violent form of delirium tremens, who, not tolerating opium, and the valerianate of zinc, together with other antispasmodics, having proved ineffectual, was finally treated with the bromides of potassium and ammonium in the following formula: ℞. Potas. bromid., ammon. bromid., āā ʒ iss.; aquæ destill., f ʒ j. Of this one teaspoonful was given every hour until three doses had been given, when she fell into a gentle slumber, and slept for four and a half hours. The treatment was subsequently continued with the most gratifying results, producing at one time eight, and at another nine, hours of undisturbed sleep, from which she awoke in her usual condition of health.

Dr. G. M. Smith called attention to the statement of Dr. William A. Hammond, that the bromide of potassium was suited to the sthenic variety of delirium tremens only.

Dr. Bulkley had exhibited these salts in combination in doses of seven or eight grains each in solution. He has not a favorable opinion of the efficacy of the tinct. digitalis in this disease, but regards as a great point in its cure the ability of the stomach to retain a substantial meal.

The Academy then adjourned.

Stated Meeting, October 17, 1866. Dr. James Anderson, President, in the Chair.

MODIFICATION OF SYME'S AND PIROGOFF'S OPERATION.

Dr. Post, in behalf of Dr. Quimby, of Jersey City, exhibited a boy who, about five months ago, was the subject of an operation, performed by the latter gentleman, on account of an injury he had received by being run over, his foot being badly crushed.

The operation consisted in laying open the ankle-joint, dissecting out the astragalus, and then making a section through the os calcis, including about half the bone, and bringing its cut surface in contact with the end of the tibia, thus maintaining nearly the original length of the limb. The advantages of this over Syme's and Pirogoff's operations are, the slight degree to which the length of the limb is diminished; that, in young patients, the integrity of the tibia and fibula is preserved, leaving nothing to interfere with the growth of the bone; and that there is no danger of osteomyelitis affecting the bone.

PLASTIC OPERATION FOR DEFORMITY FOLLOWING CANCRUM ORIS.

Dr. Buck then presented a boy, nine years of age, on whom he had operated, stating that for the last three or four years it had been his especial prerogative to repair damaged faces. The patient in question (a boy nine years of age) had been in St. Luke's Hospital for some months, with his mouth in the condition shown by the cast exhibited to the Academy; the right half of the upper and lower lips being destroyed, the teeth exposed, the neighboring parts of the right cheek encroached upon by the destructive process, and a firm cicatrix binding the jaws together, so as to prevent separation of the teeth. The mucous membrane was destroyed, so as to obliterate the cavity of the cheek. The columna nasi was gone, and the right nostril a little compressed from the effects of cicatrization. There was rather more remaining of the left half of the lower than of the upper lip.

After admission to the Hospital, a sequestrum, involving one or two teeth of the right side, separated, and came away. The de-

structive ulceration was the result of cancrum oris following a low form of fever; and, what was singular, another boy, twelve years of age, had come under his charge, having the same destructive process from the same cause, but not including the same parts.

The first operation was performed some months ago in the following manner, viz. the lining membrane of both upper and lower lips, where reflected from the cheek to cover the jaw, was detached for some distance back. Both lips were then divided in a line with the ala of the nose, permitting them to be drawn to the opposite side. The edges were prepared so as to coapt them. This first operation did not fully restore the symmetry of the face, the left angle of the mouth being left in a line with the left ala of the nose. A second operation was therefore resorted to for the purpose of extending the lip on the left side, giving more symmetry. An incision was made in a line with the vermilion border with a double-edged knife. The knife was carried through the integument into the cheek, towards which the mouth was to be extended. The knife was then carried upward and downward, and the extremities of a transverse incision were united with the horizontal one by two oblique cuts which removed the integument to the extent to which the mouth was to be carried. The mucous membrane was divided to a corresponding extent, and sutures used to secure coaptation.

Wooden wedges were used between the teeth for the purpose of keeping the jaws separated.

Dr. Ordronaux then read a paper on "Malpractice in its Legal Relations."

The Academy then adjourned.

Stated Meeting, November 7, 1866. Dr. James Anderson, President, in the Chair.

TRAUMATIC NEURALGIA.

Dr. Post stated that he had recently treated several cases of traumatic neuralgia. A gentleman of the medical profession, residing in Georgia, while in this city during the winter of 1860–61, fell upon the ice, and, as he supposed at that time, sprained his wrist. In a few days afterwards he began to complain of pain in the posterior part of the wrist. He went back to the South, and remained during the war. About a year ago the pain had become so intense that he consulted Professor Dugard, of Atlanta, who made an explorative incision, and found that there had been an oblique fracture of the radius. A nerve running along the interosseous space had been encroached upon by a growth of bone, and the consequent irritation had produced a neuralgic pain. The nerve was incised; but when the wound healed the pain returned, and was more severe than before. Three weeks ago he came to

this city and consulted Dr. Post. His general health had not suffered much. The pain he had experienced had been more severe at night than during the day; and on account of this tendency to periodicity, he had taken quinine, but without benefit. He had also taken very large quantities of morphia. Preliminary to an operation, Dr. Post administered ether, but he had been so much under the influence of morphia, that the ether failed to produce an anæsthetic effect, and the doctor finally resorted to chloroform. This was the first case in which Dr. Post had been obliged to resort to chloroform. He attributed the failure of the ether solely to the use of morphia, which the patient had been in the habit of using to the enormous quantity of thirty grains daily. The operation consisted in cutting down on the posterior surface of the radius in the same line as that of the previous incision; and on exposing the dorsal branch of the musculo-spiral nerve, he found it four times its normal size and hyperæmic, so that it was very red and vascular, and on attempting to raise it from the subjacent tissue, it bled freely. An inch of the nerve was exposed and excised, which had the effect of relieving the patient; and though he still suffered some pain, he was able to reduce the morphia to six grains daily. After a few days the pain left, the inflammation having subsided.

A week after this operation, Dr. Post operated on another patient, a young soldier wounded two years ago before Richmond. He was shot through the arm a little above the elbow, the arm having been amputated in consequence. As the stump healed, he felt two tumors, one occupying the site of the median, the other that of the ulnar nerve. They being extremely painful, and very tender on pressure, Dr. Post cut down on the nerves, and dissected out about two inches of the mass, which relieved the patient after the wound had substantially healed, he at present being free from pain.

While in Colorado, some years ago, Dr. P. was consulted in regard to a patient who had a painful condition of the stump left by amputation at the upper third of the arm. A ganglionic enlargement of the median nerve was found involved in considerably hard cicatrized tissue. The enlargement of the nerve was dissected out. He found the integument across the stump shortened; and he shortened the stump by sawing off the bone. He heard from the patient a year afterwards, when he was doing well.

He had had another case within the last few days: a young lady, complaining of intense pain in the anterior crural nerve. She had suffered for some days, and at that time he found her in great agony. He used hypodermic injections unsuccessfully. Day before yesterday (Nov. 5), he applied three camphor moxas. Since that time the pain had been entirely relieved.

Dr. Bulkley, at the request of Dr. Post, related his experience in the use of hypodermic injections of quinine: stated that there

were many cases in which it was of the utmost importance to introduce quinine very rapidly into the system. In congestive fever there was sometimes great torpor of the stomach; and in such cases, hypodermic injections were often very useful. Most of the cases received at the New York Hospital are from the South.

The first case mentioned was that of a man from Aspinwall, attacked a few days before admission; came in in a state of collapse, pulseless, and unable, or nearly unable, to swallow. Dr. B. was not willing to trust to the stomach in this case, and therefore resorted to the hypodermic injection of quinine four or five times, four grains each time. In addition to this treatment, stimulants were given, and nourishment per rectum. This patient rallied, reaction soon coming on; and as far as the result of treatment was concerned, the case seemed to show that it was successful. This patient subsequently died, but from causes independent of the febrile attack.

Another man was admitted in nearly the same state of collapse, almost but not quite pulseless. In his case, also, the hypodermic injections of quinine were used to about the same extent as in the former case. Besides this, the patient took a large quantity by the stomach. Dr. B. remarked that one grain administered by hypodermic injection was equal to *four* given by the stomach, in consequence of the remedy in the former case being introduced directly into the circulation.

The third case was not quite so severe. In this case, three or four hypodermic injections were used, and the patient recovered. Dr. B. stated that Dr. G. M. Smith had used it in common intermittents. In two or three instances, by the use of four grains in this way, just previous to the paroxysm, he had succeeded in preventing it; giving it in the strong solution of seven grains to the drachm of water, every thirty-five minims containing about four grains of sulphate of quinine.

This was not an entirely new plan of treatment. He had seen some account of its being used in the East Indies; was not aware that it had been used very much in this city. He would mention that in the three cases mentioned above there were no unpleasant consequences. On some former occasions slight ecchymoses had occurred; and on one occasion an abscess was formed.

Dr. Post stated that the ordinary process of medical men using these injections was to insert the syringe and throw the solution in one body into the tissue, lacerating it, and leaving a solid mass. His own practice was to throw in a drop or two, and then push the instrument a little further, continuing this process until the desired amount was injected. By that means the fluid was diffused, and laceration of the tissues avoided. This was a matter of considerable importance, as regarded the tolerance of the treatment.

Dr. Elsberg inquired whether any member of the Academy had used hypodermic injections of strychnia, stating that it had

been employed in Europe to the amount of one-fortieth to one-twentieth of a grain—for local paralysis, for example—in paralytic affections of the vocal chords.

Dr. Wooster remarked, in regard to muriate of morphia, that it did not produce nausea like the other preparations, particularly the sulphate, when taken into the stomach.

Dr. Bacheldor had used the muriate of morphia in place of the sulphate or other preparations, for ten or eleven years past, and considered it the most perfect anodyne employed. He carried it constantly, in the form of pills, of one-fourth of a grain each, a specimen of which he exhibited to the Academy. He remarked, in reference to neuralgic pains, that they commenced about the spinal marrow, and continued there some time, until the pain became excessive in the part to which the nerve is distributed, after which, the trouble in the spinal marrow ceases. It had been his practice in local neuralgic pains to examine the spine, and generally found tenderness at the point of origin of the nerve supplying the painful part. The application of a blister, seton, or leeches, at that point, often stopped the pain. He mentioned the case of a patient, sent to him from a distance some forty years ago, complaining of great pain in the little, and one-half of the ring finger, and extending up the arm a little above the wrist. This patient, a lady, suffered very much in the night. Had been seen by several physicians, and almost every means of relieving pain had been tried. On examining the neck, and passing the finger down the spinous processes, he found one exceedingly tender; and pressure at that particular point caused pain in the neuralgic part. He applied a blister, and the pain ceased almost before the blister had done drawing. He would suggest the use of quinodine in congestive fever. He found it most efficacious in stopping fever and ague; had given it in doses of two grains three times a day; did not know one case where the second paroxysm had occurred, it having relieved every case he had seen.

Dr. Elliot stated that he had had a large experience in the hypodermic injection of sedatives. He had published the first two cases ever published in this country, in the *N. Y. Journal of Medicine* for November, 1858. Employed this remedy in Bellevue Hospital. Two very interesting cases of sciatica were cured by this remedy alone. Within the last two years he had used it frequently, but in a smaller proportion of cases than formerly. This was not because the remedy had failed in its therapeutic effects, but because, in some cases, it had led to evil consequences, among others, of self-injection, to produce the intoxication of opium.

The Academy then adjourned.

STATED MEETING, NOVEMBER 21, 1866. DR. JAMES ANDERSON, PRESIDENT, IN THE CHAIR.

DISCUSSION ON CHRONIC METRITIS.

DR. BUDD, by appointment, opened the discussion as follows:—

MR. PRESIDENT—Of all the diseases to which the human female is liable, there is none, in my opinion—save perhaps with the exception of various forms of malignant affections—which so generally baffles the practitioner as the one under consideration. This is, in a great measure, dependent upon, and attributable to the fact of the periodical physiological congestion to which the uterus is subject.

Chronic metritis, chronic inflammation of the parenchyma of the womb, may be either partial or general. It may affect the entire parenchymatous tissue of the womb; or it may be confined to a part of it. When it is partial, it is generally confined to the posterior portion of the uterine walls, which is attributable to the fact of the peculiar anatomical arrangement of the muscular fasciculi which form the organ. The neck and the body of the womb are almost as distinct as two separate organs, deriving their vascular and nervous supplies from two separate sources. In the body of the uterus, we find the muscular fibres arranged in various forms, being concentric around the cornua, oblique and longitudinal in the central portions. In the neck they are circular; and it is only in the posterior part of the organ that the longitudinal fibres are continued from the body into the neck. This is the only anatomical connexion, so far as the muscular fibres are concerned, that exists between the body and the cervix. But the various physiological functions are also very different. The fibres of the body and fundus contract during parturition, while those of the cervix dilate. My object is to demonstrate that chronic metritis depends on one of two causes—either the direct result of acute metritis, puerperal or otherwise, or it is dependent on inflammatory action, which has existed in the cervix, propagated through the long fibres which form the muscular connexion between the body and cervix, and which, as before observed, exist most abundantly in the posterior part of the organ. This antecedent inflammation may exist for years before it is propagated to the body through the posterior longitudinal fibres. The situation of the posterior longitudinal fibres accounts for the clinical fact that inflammation of the posterior part of the uterus is more common than of the anterior.

There is another point upon which gynecologists are at variance: whether endometritis occasions chronic parenchymatous metritis. Is inflammation of the womb the result of an original endometritis? Rokitansky thinks it is. If we examine the

anatomical relations of the lining membrane of the uterus, we shall readily comprehend how the tissues of the uterus can be brought to participate in inflammation affecting the surface. We find the closest connexion between this lining membrane and the tissue proper; and we are certain that there is no submucous areolar tissue. In the body of the womb it is impossible to separate this lining membrane from the parenchyma. It is not so with the cervix, where the membrane can be easily dissected off and separated. We frequently find endometritis of the catarrhal form, where the substance of the womb does not participate in the inflammation; but we seldom find the parenchyma of the womb inflamed, where the mucous structure does not participate in the morbid action.

In regard to the pathology of this disease, it exhibits a peculiar tendency towards exacerbation during the existence of the molimen hæmorrhagicum which precedes menstruation for two or three days. During the menstrual interval the symptoms subside. This is due to the fact that an organ, already the seat of inflammation, has periodically added to it the congestion attendant on the menstrual function. The relief afforded by menstruation is due to the physiological depletion which occurs.

The diagnosis of this disease is simple and satisfactory; the uterus is enlarged and tender, the slightest pressure being attended with great pain. The general symptoms are those that attend all uterine affections, particularly deranged digestion. The progress of the disease, as a rule, is slow and steady; and its termination generally not by resolution. Its termination is generally delayed till after the climacteric period has occurred, and the menstruation become permanently arrested.

In reference to the therapeutics of this complaint, it is exceedingly unsatisfactory. We can relieve, mitigate, palliate; but the cases where we can cure are rare.

The treatment resolves itself into local and general; the former consisting in depletion and counter-irritation, and the latter in the use of alteratives and tonics. Local depletion, as a rule, is followed by decided relief, which is more decided if used just before a menstrual period than during the interval. Free scarification of the neck is sufficient to relieve the symptoms without carrying it so far as to affect the general condition of the patient. Depletion, whether by leeches or scarifications, generally produces benefit. Cups over the sacrum generally give relief. Counter-irritation may be applied to the cervix, or to the hypogastrium. I frequently use vesicating collodion to the cervix uteri. Where endometritis is present, I resort to the tincture of iodine, applied to the fundus through the agency of a hard rubber or flexible probang, adapting itself to the cavity through which it passes. Setons, in the hypogastric region, or issues over the sacrum, are often of benefit, as well as the actual cautery, in cases attended with hypertrophy of tissue. As to general treatment, the

administration of mercury, in small doses, appears to be most satisfactory. I usually give the bichloride, combined with the compound tincture of bark. I have alluded to partial, as distinct from general chronic metritis. Under these circumstances, we have displacement to complicate the original morbid condition. Where the posterior part of the organ is involved, we have retroversion in consequence of the increased weight due to the hyperæmia. Retroversion is rarely uncomplicated with this condition of hyperæmia of the posterior part of the womb. Gynecologists differ in regard to the propriety of using mechanical supports to the womb under these circumstances.

Dr. Peaslee said:—

Mr. President—The subject of metritis is surrounded by greater obscurity than any other in uterine pathology. Almost all writers agree that it is a very common disease. The statement was made by Dr. Bennett, of London, about twenty years ago, that it embraced eighty per cent. of all cases of uterine diseases; and that seventy per cent. of these are complicated with ulceration. The statement was also made by Dr. West, five years afterwards, that it was very common; but he did not say it was so often attended by ulceration. Scanzoni also states that it is very common; but he uses two terms, chronic *parenchymatous metritis*, and *chronic engorgement*, as synonymous.

It is well to inquire what chronic metritis is. No one will now affirm that eighty per cent. of all uterine ailments, nor twenty per cent. probably, are metritis of either form.

We will suppose a case presenting the symptoms of nausea, great pain in the uterus, with a sensation of weight; thirst, loss of appetite, constipation; pulse indicating high febrile excitement. These are symptoms of acute inflammation of the uterus. If it is a first attack, we certainly should not call it *chronic metritis*. If the patient has had other attacks, which have been relieved, can we call it *chronic* metritis? We see the patient a fortnight afterwards, and find the functions partially restored; some pain on walking, some tenderness; no febrile excitement, no thirst, or constipation. But the patient, though partially relieved, is by no means well. She is pale and anæmic, and looks miserable. This may go on for years; a similar train of symptoms attending the menstrual periods. Is this chronic metritis? Not so for me. I do not know that I have ever seen a case that I should call chronic metritis; but *recurrent* metritis is common. I should make a distinction between recurrent metritis and chronic congestion; and nine out of ten so-called cases of chronic metritis are nothing but chronic congestion. This congestion is, of course, increased at every menstrual period, and is liable to pass into inflammation.

A recent writer on chronic metritis makes the curt statement:

"Tender uterus is diseased uterus, and in most cases is inflamed uterus." Celsus gives the signs of inflammation—"redness, swelling, heat, pain;" but all these occur in congestion too. In my experience, I find many uteri tender; and but very few inflamed. I do not regard the heat, pain, or sensibility, as symptoms of inflamed uterus, unless accompanied by febrile excitement. Nor does inflammation occur in any important organ of the body without this general symptom, except it be extremely limited in extent.

It has been repeated here, that a partial inflammation frequently attacks the posterior wall of the body of the uterus; but I think that such limited inflammation is very uncommon. This is merely the part most easily reached by the finger; and in which tenderness or swelling is easily detected. In cases of retroflexion, therefore, the posterior wall, and in anteflexion the anterior, is found to be tender; while the rest of the organ cannot be reached. But this does not decide between inflammation and congestion; and we must admit partial congestion as well as partial inflammation.

I make this distinction between the two conditions. When febrile excitement is absent, when the patient remains in about the same condition year after year, my idea is that it is not chronic inflammation, but simple chronic congestion; which may pass at times, or may not, into inflammation. It is important to make this distinction between the two conditions, for an erroneous diagnosis may lead to important mistakes in treatment. If we keep the idea that we are dealing with inflammation, depletion at once suggests itself, and we are apt to carry the depletion too far.

Dr. Bennett uttered a caution twenty years ago which is calculated to lead to false impressions in reference to this form of uterine disease. He says: "You may mistake this inflammation for a displacement which was caused by it." This leads to two erroneous inferences: one is that a displacement is always dependent upon inflammation, which is not the fact in the majority of instances; and the other is, that you should always treat the inflammation instead of the displacement. In many cases we cannot cure the inflammation till we remove the displacement.

In regard to the therapeutics of this affection, we shall find less difficulty in subduing the inflammation than in relieving the congestion. In the treatment of this class of affections my experience is not satisfactory, unless certain conditions can be observed. In the first place, I deem it necessary that the patient shall not stand nor walk much, though she should ride much in the open air. I consider it important that the skin should be kept in a healthy condition, and that the alimentary canal should be kept free from derangement. Saline laxatives may be used to remove constipation. The state of the liver should be attended to. Mercurial alteratives may be used. Furthermore, the patient should be

kept free from excitement, and particularly from marital intercourse. Still, judgment should be used in reference to the latter point. It will often be advisable to send the patient abroad. It is this class of cases that are frequently benefited by being sent to "water cures." In regard to local treatment, my practice has been, without much regard to the general condition of the patient, to take blood from the uterus, but a very small amount at a time, by scarification; or rather to make a crucial puncture with a spear-shaped scarifier. I take usually but a drachm to four of blood, but I repeat the local depletion every two or three days, according to the condition of the patient. The bleeding may be checked, if excessive, by applying the persulphate of iron. As an alterative I prefer the bichloride of mercury. I also frequently administer the bromide of potassium, six to ten grains, three times a day.

Dr. Barker said:

Mr. President—I fully agree with the gentleman who opened the discussion, in regard to the anatomical and pathological conditions of the uterus. He probably forgot to allude to one point of much importance, viz. the peculiar character of the uterine tissue itself. It is an organ of low vitality, resembling cartilage, or bone, rather than muscular tissue while in its non-gravid state. This condition entirely changes during the period of pregnancy, as well as in menstruation. I must confess that I have listened with astonishment to the pathological views of the gentleman who has just sat down. They may be entirely correct. I can only say they are directly opposite to mine. He makes a distinction between inflammation and engorgement. I agree with him that tenderness of the uterus does not always indicate inflammation, but not that the latter is always attended with febrile excitement. Every surgeon sees inflammation of bone and cartilage continue for a long time without febrile excitement. In inflammation of the uterus, does not the fact that the tissue involved entirely changes its character, explain the great difference in symptoms during a menstrual period and the menstrual interval? The distinction between the pathology of disease of the cervix and body of the uterus is, in my opinion, very important. I do not think chronic metritis ever reveals itself by the speculum, or that any change in the cervix is indicative of inflammation in the body of the organ. The treatment of inflammation of the neck and of the body of the uterus is entirely different.

In making a diagnosis of chronic metritis we must bear in mind that in the healthy uterus, if we make a rectal examination, we find the posterior wall of the uterus smooth and regular. In parenchymatous inflammation we find this surface nodulated and irregular, and painful on pressure. This is especially the case during menstruation. We find this inflammatory condition often

associated with induration, and more rarely with ramollisment. The latter condition is a result of uterine phlebitis.

Chronic metritis never subsides spontaneously during menstrual life; but a large majority of cases get well after the cessation of menstruation. Pregnancy may effect a cure, because parturition is followed by involution and breaking down of the interstitial deposit in the muscular walls of the uterus.

In regard to treatment, I have found depletion of no permanent benefit, though I sometimes scarify the os uteri; thus depleting not the organ affected, but one contiguous to it. I have used the proto-iodide of mercury in half-grain doses, combined with sulphate of iron and a small quantity of opium, or some of its preparations, to check any tendency of the proto-iodide to irritate. I am in the habit of using large injections of hot water into the pelvic cavity, especially in that form of chronic metritis associated with induration—chronic parenchymatitis, connected sometimes with dysmenorrhœa, sometimes with menorrhagia. The effect of hot water in liquefying (if I may use the term) plastic exudation is astonishing. It is now four or five years that I have been in the habit of using it. If any success is to be obtained from it, it is necessary for a very large quantity to be injected at each time. I direct a sheet of rubber cloth to be spread over the front of the bed. The patient is brought to the edge of the bed, and placed in the position used for obstetric operation, her hips resting on the rubber cloth, the lower edge of which passes down into a pail half full of hot water, placed on the floor in front of the bed. The water is injected by the ordinary syringe, and is passed back down the rubber sheet into the pail below. In this way I inject fifteen or twenty gallons of water at a time, continuing it for fifteen or twenty minutes.

The effect of this treatment in producing absorption of plastic exudation, in liquefying it, so to speak, is marked and extraordinary. In that form of chronic metritis associated with dysmenorrhœa, I have used it before each menstrual period with the effect of greatly increasing the menstrual discharge.

Dr. B. here exhibited an instrument used for injecting fluids, or ointments, into the uterine cavity.

The Academy then adjourned.

STATED MEETING, DECEMBER 5, 1866. DR. JAMES ANDERSON, PRESIDENT, IN THE CHAIR.

CHRONIC METRITIS.—(CONTINUED.)

DR. KAMMERER remarked as follows:

MR. PRESIDENT—In the discussion at the last meeting of this body, the question was asked whether there was such a thing as *chronic metritis;* one of the gentlemen using this expression,

another objecting to it. I believe that this difference of opinion arises from the fact that pathological and morbid anatomy are far in advance of clinical observation. When we speak of inflammation, chronic inflammation of the uterus, this brings us to inquire, "What is *inflammation?*" As I understand it, *inflammation* is a derangement of nutrition in the tissue, characterized by exudation and stagnation of the capillary circulation in the part affected. Hyperæmia is not inflammation; for if it were, the womb must be inflamed every four weeks. Neither is irritation inflammation, for the organs may be irritated and not inflamed.

Now the question is—Is there inflammation of the body of the uterus? There is no doubt about the existence of metritis; there is no doubt about the existence of endometritis; but as to the matter of inflammation of the body of the womb, it is necessary to speak with some reserve. Indeed, French authors, and some of the older German authors, describe the pathological condition of the uterus which they consider genuine inflammation. They say, "The uterus is larger, of a deeper color, more succulent and congested." However, these cases are pronounced exceedingly rare.

The best writer on this subject in Germany—a pupil of Rokitansky, whose work I am sorry to say is not read on this side of the Atlantic—says (and this is saying a great deal for one who has made many of those autopsies): "I have been unable to meet with a single case where the uterus was the seat of genuine inflammation." All those he found were simple indurations, resulting from chronic hyperæmia.

Now, what does the clinical pathologist teach?

A patient seeks advice, complaining of certain functional derangements, amenorrhœa, dysmenorrhœa, menorrhagia, etc., certain objective symptoms referable to this. A speculum examination is made, and the examiner finds erosions, etc. He goes further; examines by the double touch, palpates, examines the size of the uterus, introduces his hand, palpates; and with the sound finds the uterus is enlarged, its cavity deepened, walls thickened.

I think the only symptom on which the practitioner can base his diagnosis is the *enlargement;* all the other objective symptoms do not necessarily indicate inflammation.

It was stated at the last meeting of the Academy that a tender uterus was not necessarily an inflamed uterus; and, indeed, it is very doubtful whether the pain has anything to do with the inside of the uterus. Moreover, we all know that there are certain forms of hyperæsthesia in females, without inflammation.

Now, the pathological anatomist only recognises two forms of enlargement of the uterus. 1st. Genuine hypertrophy; the organ has its normal shape, perfectly natural, fulfilling all its normal functions. This is no disease, but perfectly in accordance with health. Engorgement, or chronic induration, is entirely different; the muscular fibres are increased, compressed, and partially destroyed—undergo fatty degeneration; the capillaries are also

destroyed. The uterus is hard, so hard that when cut it creaks under the knife. Now, this is not a necessary consequence of inflammation; this is no morbid process, but a morbid condition, habitual congestion—hyperæmia existing for a long time. We find this in an arterial form, for instance, in excessive sexual intercourse; we find it in menstruation; in the puerperal condition. There is another form of congestion, which is called the *passive venous* congestion, arising from difficulties in reflex blood. This we see in all displacement conditions of the uterus, under certain conditions of the general system, in acute diseases of the heart, small-pox, tuberculosis, typhus fever, etc. When this becomes habitual, we have engorgement or hypertrophy of the uterus.

As to the treatment of this engorgement, or chronic metritis, my experience is that the main things to treat are the complications. All agree that hypertrophy of the uterus is almost never found without secretion from the cavity. A great many cases coming under our notice are complications of the peritoneum: chronic peritonitis is a very frequent accompaniment. I make it a rule to remove all peritonitis first, by means of antiphlogistics, of which I think the tepid bath is the best. As far as chronic endometritis is concerned, I have made it a rule first to keep freely open the canal, and for this purpose we have the sponge-tent, the laminaria digitata, and the bougies (the latter of which I have had made a special set of different sizes). Then, the cervix being well dilated, so that there is a free exit for the secretions, this is usually sufficient to give the patient much relief by providing for the escape of the fluid. Very often we have contraction of the cervix, or closing of the inner os, and these agencies will serve to keep it open. If internal remedies are to be applied, I generally resort to the liquid form. Of the many remedies long in use, I will only mention two which are more or less known, and perhaps many are better than these: pyroligneous acid, much used in Europe, much used by Meyer, of Germany; and facts are established that it can be used without any danger whatever, if the os be kept well opened. The remedy which I prefer is carbolic acid—concentrated solution—which I use almost every day, and from which I have not, to this day, seen any unfavorable consequences (applied with a brush). However, it must not be used where there is peritoneal or acute inflammation, or where there is any peritoneal complication.

The positive relief we can give a woman thus suffering seems doubtful; but we may console by telling her that parturition will put an end to her trouble, that she will be entirely relieved at the cessation of menstruation, etc.

Dr. Peaslee spoke as follows:

Mr. President—My remarks respecting chronic metritis, so called, at the last meeting, have been somewhat earnestly criticised,

as I hoped they might be; and I regret that Dr. Barker could not be present this evening, as he might wish to reply to criticisms I may in turn feel obliged to make. I have held this view for fifteen years, and think it can be maintained as both logical and scientific.

It is a very common error to confound the sequelæ or results of inflammation with inflammation itself. Dr. Barker recognised three forms of chronic metritis, viz. (1) when complicated with induration; or (2) with softening; and (3) another complication which I do not recollect. Churchill makes the same classification, with two or three additions.

But induration is merely a result of the organization of the plasma which is exuded by a preëxisting inflammation; and softening is the immediate result of the exudation itself into the intercapillary spaces of the inflamed part. An exudation takes place in every inflammation, and may be disposed of as follows: (1) It may be reabsorbed, and the inflammation is then said to end in resolution; (2) it may become organized, and after a time the part (if a soft solid) becomes harder than before (induration occurs); or (3) it may degenerate into pus (or suppuration supervenes). But so long as the exudation remains unchanged in the part, it is of course *softer* than usual. The axiom of twenty-five years ago, therefore, that acute inflammation produces softening, while chronic inflammation produces induration, expresses a very superficial view of the facts. Induration is a *chronic* result of inflammation, whether acute or chronic; and if inflammation coexists with it in any case (or with softening), its treatment is the same as if independent of it.

Another error I would correct is, the confounding of the terms engorgement and inflammation as if synonymous. If we produce inflammation artifically, we first produce a rush of blood to the capillaries (local determination of blood); second, then an accumulation of too slowly moving blood (congestion); and third, finally an exudation of plasma into the inter-capillary spaces (inflammation). Engorgement simply implies a crowded condition of the vessels or the tissues of a part, or both; and sometimes, therefore, means congestion simply; while it may also indicate the *condition* of a part after an exudation has taken place. But it never means a *process* (an inflammation), and the sooner it is dropped from the nomenclature of pathology the better.

Another point to which I would call attention is the assertion, of late quite fashionable, that the cervix uteri and the body are to be regarded as independent organs; and which is entirely incorrect, whether we consider their relations in a developmental, a histological, an anatomical, a physiological, a pathological, or a therapeutical point of view.

1st. *Developmentally*, they are one and the same organ. The internal genital organs of the female are developed from the ovaries, as those of the male are from the testes. One-half of the

uterus (cervix and body) is developed from each ovary, and these finally meet and blend on the median line.

2d. *Histologically*, they have the same tissues (the body having also a peritoneal investment). Dr. Barker suggested a similarity, histologically, of the uterus to cartilage and bone. But cartilage has no vessels or nerves—nothing but cells and homogeneous substance; and bone has no muscular fibre, nor anything else that is found in the uterus, except nerves and vessels. No histological analogy is therefore to be discovered in the organs he compares.

3d. *Anatomically*, they are the same organ. The longitudinal fibres of the body are prolonged into the cervix anteriorly as well as posteriorly, as I had occasion to state ten years since in my work on Histology. Otherwise, in every case of parturition with rigidity of the cervix, the fœtus would be expelled into the peritoneal cavity, through a rupture between the cervix and the body in front.

4th. *Physiologically*, the cervix is distinct from the body in some respects. But in respect to the great function of parturition, they are one and the same organ; the cervix being the sphincter muscle of the body.

5th. *Pathologically*, the cervix and body are not distinct organs. For though endometritis may exist without endocervicitis, and metritis without cervicitis, and both these propositions may be reversed also; yet irritation of the cervical canal by a sponge-tent may produce endometritis (or abortion), and the common cause of metritis in the newly married is contusion of the cervix.

6th. Finally, the cervix and the body of the uterus are not distinct organs, *therapeutically* considered. If so, and no application to the cervix modifies the condition of the body, all local treatment of the class of cases under consideration must be abandoned as futile. But Dr. Barker admits that taking blood from the cervix, as practised by myself, gives relief, though he thinks it but temporary. The body and neck are, however, supplied with blood as well as with nerves from the same source. Besides, Dr. Barker places his main confidence, so far as local treatment is concerned, in vaginal injections of warm water—a practice quite irrational, if the body and neck are not therapeutically the same organ; but which I also sometimes resort to, on the principle that they are so.

Dr. Kammerer has stated that he did not know a certain remedy for the induration in such cases. I cannot shed much light on that subject, but think we must for the present trust mainly to the bichloride of mercury, the bromide of potassium, and especially to *time*.

DR. QUIMBY'S MODIFICATION OF SYME'S AND PIROGOFF'S OPERATION.

Dr. Post, on behalf of Dr. Quimby, again presented the case of the boy upon whom the latter gentleman had operated, exhibit-

ing the patient before the Academy. On this occasion the patient was wearing an artificial front foot, consisting of an ordinary boot with a steel plate extending from the heel to the toe, being fastened to the sole of the boot; the front part of the boot being filled with rubber tubing, and the instep formed by a hollow rubber ball; the entire length of the boot-leg being open, and made to fit closely by lacing.

With this simple contrivance the boy was enabled to run and jump with great facility, much better than those wearing artificial limbs after Syme's operation. The boy was presented the second time to show that the objections raised by some to this operation—that the stump was too long, and would prevent the wearing of an artificial foot—was altogether futile and erroneous, as the boy was able to walk with great ease and facility, at the same time the feet appearing perfectly symmetrical.

Dr. Quimby was desirous of calling the special attention of the profession to the fact that, in the three cases in which he had performed this operation, the incrusted cartilage of the tibia and fibula, or their relations, had not been disturbed in the least, but carefully avoided, the calcaneum being brought in direct contact with the cartilaginous surface.

He wished also to correct the false impression entertained that he scraped off or denuded the cartilaginous surface, which was not the case, as he considered the cartilaginous surface of great advantage in the success of the operation, believing that it assisted in forming bony or cartilaginous union between the parts.

The Academy then adjourned.

Adjourned Meeting, Dec. 13, 1866. Dr. James Anderson, President, in the Chair.

The Academy convened this evening for the purpose of listening to the following paper:—

VIVISECTION:

WHAT IT IS,

AND

WHAT IT HAS ACCOMPLISHED.

BY JOHN C. DALTON, M.D.,
Professor of Physiology in the College of Physicians and Surgeons, New York.

Mr. President and Gentlemen of the Academy—As the subject of vivisection has been recently brought to the public notice in a way calculated to excite especial interest in its merits and demerits, I have thought it appropriate to make it the topic of a brief address, and to examine the question of its propriety and usefulness, as a means of improvement in the medical art. For this purpose, I propose to discuss the objections which have been urged against it, and to show how far they are valid, and how far destitute of foundation.

The objections urged against vivisection are principally threefold; viz.:

First, that it is cruel;
Secondly, that it is liable to uncertainty and deception; and
Thirdly, that, in point of fact, it has not led to valuable results.

These three objections may be considered in succession.

First, as to its cruelty. The injustice of this charge may be appreciated, when we remember the aim and motive of all such experiments. Their object is the improvement of medical knowledge, and consequently the relief of human suffering and the cure of human diseases. Physiologists have sometimes been charged with recklessness and disregard of the lower creation in following out their passion for experiment; and it is said of one of them that he destroyed, in the course of his life, "no fewer than three hundred and seventeen animals," with this object. Now if we destroy, every year, four hundred thousand cattle in South America* for no other purpose than to supply ourselves with boots and shoes, it does not seem a very reckless or extravagant thing to sacrifice a comparatively small number of dogs and rabbits for the

* The number of hides imported into the United States, during the year 1865, from Buenos Ayres alone, was 400,978. From the whole of South America, 525,438.

acquisition of knowledge which is to benefit the human race. This is a legitimate and praiseworthy object; and experimental vivisection is no more open to the charge of cruelty on this account, than the dissection of human bodies for the study of anatomy is open to the charge of sacrilege and impiety.

Besides, it can never be properly the aim of the physiologist to inflict unnecessary suffering. On the contrary, it is desirable on every account to avoid doing so. No class of men can be more averse to the infliction of suffering than the physicians and naturalists who make the functions of the animal organism their special study; and none are better qualified to judge of the existence and degree of sensibility in particular cases.

It is perfectly true that vivisection may be and has been abused, in certain instances, by reckless, unfeeling, or unskilful persons. But this abuse of a practice, which is rare and exceptional, cannot with any reason be urged against the practice itself when conducted with judgment and propriety. I have myself witnessed this abuse, especially in the case of a veterinary institution in a foreign country, where the pupils were allowed to practise surgical operations on the living and sensitive animal, for the purpose of learning the operative manipulations. Nothing could be more shocking than the performance of these operations. Many of them were evidently cruel and unnecessary, and excited the hearty condemnation of all who visited the place. But this practice had nothing to do with physiological investigations. It was confined to veterinary institutions abroad; and neither at that time, previously nor since, so far as I am aware, has it ever been in any way resorted to in this country.

It is also proper to remember that the great majority of cases of painful operation on animals, for physiological experiment, date from a time anterior to the discovery of ether and chloroform, when surgical operations on the human subject were equally painful. Since the discovery of anæsthetics, their use has been adopted by the physiologist as promptly as by the surgeon; and in point of fact, at the present day, the great majority of physiological experiments are so performed as to be entirely painless. The protecting influence of anæsthetics, when fully administered, is absolute and complete, and the most extensive incisions may be performed without the animal experiencing the least consciousness of the operation. The infliction of pain, therefore, as a general rule in physiological operations, has no existence, and does not apply as an objection to the practice.

But there is a very small class of cases in which anæsthetics cannot be used; and these cases present a difficulty to the minds of some, who are ready to acknowledge the propriety of vivisection in other instances. For here, at least, the animal must feel. Not being etherized, he remains sensible; and that degree of pain which naturally attends the experiment cannot, in such a case, be avoided.

Now the difficulty which prevents the use of ether, in these cases, is a perfectly simple one. They are cases mainly of experiment upon the nervous system, in which it is required to ascertain the existence or non-existence of sensibility in particular parts. Of course, therefore, if the sensibility of the animal were already suspended by ether, there would be no means of judging of its existence or otherwise; and the operation could have no possible result. If the experiment be done at all, therefore, it must be done without the use of ether.

But a little examination will show how small, in reality, even in these cases, is the amount and frequency of unavoidable suffering; and how exaggerated are the pictures which have sometimes been drawn of their harshness and inhumanity. For, to begin with, it is not all experiments, even on the nervous system, which are accompanied by pain. Contrary to what might be anticipated, observation has shown that many of the nerves and nervous centres are insensible, and cause no pain when exposed and manipulated. Sir Charles Bell noticed many instances of this. In speaking of the two classes of nerves, and the sensibility of one of them, he says: "While, on the contrary, nerves not of this original class or system are comparatively so little sensible as to be immediately distinguished, insomuch that the quiescence of the animal suggests a doubt whether they be sensible in any degree whatever."*

Again he says, in the same connection: "If the fifth nerve and the *portio dura* of the seventh be both exposed on the face of a living animal, there will not remain the slightest doubt in the mind of the experimenter which of these nerves bestows sensibility." †

Also in reporting his *Experiments on the Nerves of the Face:* "An ass being thrown, the portio dura was divided on one side of the head. On the division of this nerve, the animal gave no sign of pain."‡

Again: "This experiment of cutting the respiratory nerve of the face, or *portio dura*, gave so little pain, that it was several times repeated on the ass and dog, and uniformly with the same result."§

In describing still another experiment, he says: "In cutting the *portio dura*, it was not evident that the animal suffered any pain at all."‖

This fact has also been often observed with regard to the pneumogastric nerve, which may be divided in the middle of the neck, without showing any sign of sensibility whatever, or one only of a very obtuse character.

* Bell, Nervous System of the Human Body, 3d Ed., London, 1844, p. 42.
† Ibid. p. 42.
‡ Philosophical Transactions. Vol. XXXI., p. 412.
§ Ibid. p. 413.
‖ Ibid. p. 417.

The same thing is true with regard to a large portion of the nervous centres. No fact is better established in physiology than that the entire substance of the hemispheres of the brain, the cerebellum, the olfactory ganglia, the corpora striata, and the tubercula quadrigemina, are completely insensible. Consequently when the cerebral lobes of a bird or quadruped are wounded or removed, this is an operation which produces absolutely no pain whatever. The truth is, that ordinary sensibility is localized, like other nervous functions, in particular parts of the nervous system, and is not to be found elsewhere. Accordingly, the experiments which have demonstrated this truth are doubly valuable: first, by showing the fact that sensibility is so localized in particular parts; and secondly, by showing upon what portions of the nervous system we may continue our investigations, without the production of pain to the animal.

Besides, in these cases of experiment on the nervous system, the preliminary operation of cutting through the flesh and subjacent parts may, in most instances at least, be performed under the influence of ether, and the final examination alone take place after the animal has recovered from his etherization. This reduces the actual infliction of pain to very narrow limits, both in respect of degree and duration.

This may easily be appreciated from what sometimes occurs in medical practice. Suppose we have a patient suffering from apoplexy and partial hemiplegia, and we wish to know whether sensibility remains unimpaired in the affected part. In order to determine this, we do not require to torture the patient, nor to subject him to any barbarous or extensive injury. Just so much impression is required as will reveal the presence or absence of sensibility; and, when that is done, the work is accomplished. Now the experiments upon the nervous system of animals, to which I allude, are of precisely the same nature. Their object is the same, and the means employed often identical. When sensibility exists, the pain which indicates it is momentary in duration, and need not be excessive or violent in degree; when it does not exist, of course there is no pain whatever.

I believe, therefore, that vivisection, when practised with care and judgment, is not open to the imputation of cruelty; but that both the objects for which it is undertaken, and the manner in which it is performed, place it beyond the limitations of such a charge.

The second and third objections which have been enumerated may be best examined in connexion with each other.

In order to place the matter in its proper light, let us therefore consider as briefly as possible: first, what experimentation upon living animals is; and second, what it has accomplished.

First of all, this method of study comprises all varieties of experiment performed upon living animals. The term "vivisection" does not fully express its character; for, although cutting opera-

tions are often a part of the means employed, they are not always so, and the essential nature and objects of the investigation are the same in either case. It makes no difference, of course, either for or against the propriety of this method, whether we experiment upon an animal by cutting his carotid artery, or by confining him in an atmosphere of oxygen or carbonic acid. The particular means employed vary, of necessity, with the special object to be accomplished. But, in all instances, the distinguishing character of experiments of this class is simply that they are performed upon the living body; and this distinguishing character is the same, whether we operate upon the animal by means of incisions and ligatures, by subjecting him to the respiration of particular gases, by confining him to a regimen of food, peculiar either in quantity or quality, or by the administration of drugs and medicines. The subject of discussion is not, therefore, vivisection in its narrowest sense, but the entire method of experiment upon living animals, as a means of study in physiology and the kindred sciences.

Now it may be stated, to begin with, that this method of investigation is the only one by which physiology can be successfully studied. This fact is now perfectly well settled by the results of long experience, and many attempts to accomplish the same end by other means. Physicians have often endeavored to guess at the vital properties of the internal organs, or to infer their functions from analogy; but these attempts have nearly always failed of their object, and have often resulted in complete deception. The physiologists who relied upon such means have been frequently misled in a direction exactly contrary to the truth; and the real solution of the question has only been reached afterward by a resort to direct experiment on the living body. In 1809, the anatomy of the spinal nerves and their double origin, by anterior and posterior roots, from the spinal cord, was already known. At that time, Alexander Walker conceived the idea on theoretical grounds that the anterior roots of these nerves were sensitive in their function, and the posterior roots endowed with the power of motion. It was only afterward, when Sir Charles Bell, Magendie, and others, experimented directly upon these parts, in various animals, that Walker's surmises were found to be exactly wrong; and that the anterior roots are in reality the organs of motion, and the posterior roots the organs of sensibility. After the discoveries of Lavoisier, in 1780, in regard to respiration, it was thought that the blood became heated in passing through the lungs. But subsequent experiments made it evident that the lungs are not perceptibly warmer than the other internal organs; and Bernard,* many years afterward, by carefully introducing thermometers into different blood-vessels in the living animal, showed that, in point of fact, the blood is a little warmer on the right side of the heart

* Cl. Bernard. Liquides de l'Organisme. Paris, 1859. Vol. I., p. 110.

than on the left, and accordingly that it becomes cooled rather than heated in its passage through the lungs.

These are some instances of the inefficacy of surmises or analogical inferences as a source of knowledge in physiology, when compared with direct experiment.

Now the reason why experiments on living animals are the only source of certain knowledge in physiology is a very simple one. Physiology is the study of the vital actions of the living body; and these actions do not take place anywhere but *in* the living body, and consequently cannot be observed or investigated anywhere else. A complete knowledge of anatomy may be obtained by dissection of the organs after death; but their vital functions are not to be learned in this way, because they have ceased, and cannot again be put into operation. The most remarkable facts have sometimes remained unknown or misunderstood for this reason. The lacteal vessels, for example, were discovered by Asellius, in 1622; and though they had often been seen in the bodies of recently killed animals subsequently to that time, it was thought, from their small size and the small amount of fluid contained in them, that the circulation of chyle must be very limited in quantity. But in 1856, when these vessels had been known to anatomists for over two hundred years, two French physiologists introduced a canula into the thoracic duct of living animals, continued the experiment while digestion and absorption were going on, and obtained in this way, in horses, from forty-five to fifty pounds of lymph and chyle in twenty-four hours, and in oxen from eighty to one hundred pounds in the same time,* a quantity which was never before even suspected; and revealed in this way an activity of one of the vital functions far beyond anything which could be surmised from examination of the dead body.

The chemical actions, also, which go on during life are only to be learned by their investigation in the living body. For the chemical reactions which take place in the test-tube and crucible, by the manipulations of the chemist, are modified and altered in the interior of the body by the various influences which surround them there; and it is impossible to guess what these alterations will be, until we try the experiment directly on the animal during life. Any endeavor to anticipate or infer these changes, without direct experiment, always results in a mistake, and retards our knowledge instead of advancing it. It is perfectly true that examinations of the dead body and ordinary chemical experiments are exceedingly useful as preliminaries, by suggesting probable ideas or supplying convenient materials; but the final question must always be decided by an examination of the functions as they actually take place during life.

* Colin. Physiologie Comparée des Animaux Domestiques. Paris, 1856. Vol. II., p. 100.

It is therefore certain that if our knowledge of physiology is to be advanced at all, it must be by means of experiment on living animals; *since there is no other method upon which we can rely.* There is no other way of learning what the vital functions really are, than by seeing how they take place while life is actually going on. We might as well expect to learn the phenomena of magnetism by experimenting with substances not magnetic, as to study the phenomena of life anywhere but in the actions of the living body.

In the next place, the method by experimenting upon the living animal is capable of being employed with judgment and practised with success. This has not always been conceded to it; and the objection is sometimes made that it is, for various reasons, uncertain and fallacious, and that it is unlikely to lead, in any case, to valuable results.

The most frequent form in which this objection presents itself is that, in operating upon the living animal, we violate and disturb the vital functions to such a degree that they exhibit unnatural appearances, and so lead to erroneous conclusions. It is said that the necessary steps of the experimental operation—the incisions, ligatures, mutilations, etc.—place the organs in an unnatural condition, and interfere with the regular performance of their functions;—so that they vitiate the results of the experiment, and render them worthless or deceptive.

This objection, however, is one which certainly would never be made by a practical experimenter, for the reason that the difficulty to which it refers is always present to his mind, and one against which he guards by unremitting precautions. This is the very first and simplest lesson which is learned by the experimental physiologist. We might as well object to the researches of the astronomer that the refractive power of the atmosphere distorts the rays of light passing to his telescope, and thus vitiates his observations. The astronomer is perfectly familiar with this refractive power of the atmosphere. He knows the mode of its operation, and makes due allowance for it in his calculations. So the physiological experimenter, in prosecuting his observations on the living body, sees at once that the external agents which he employs in his operations, themselves exert a certain amount of influence in producing the result. It is always his object, in arranging an experiment, to reduce these disturbing influences to the smallest possible compass. It is frequently necessary to vary the method of procedure for the same experiment, in order to determine how much, if any, of the final result is attributable to the principal conditions, and how much to the accessory manipulations. Abundant instances of this are to be found in the history of investigation upon almost every physiological question; and he would be a bungling experimenter indeed who should leave out of consideration the physical conditions which he had himself employed in operating upon the living body.

This objection, therefore, is not something which has been neglected or overlooked by physiologists; but, on the contrary, it is better understood by them than by any one else. In order to illustrate how old a thing this objection is, and how frivolous a form it has sometimes assumed, let me quote an instance which happened in connexion with the discovery of the circulation of the blood. In the time of Harvey, two hundred and forty years ago, it was believed that the blood in the veins moved from the heart outward toward the extremities. Harvey asserted that it moved in exactly the opposite direction, viz., from the extremities toward the heart; and the proof of this was that when a vein was divided, while the circulation was going on, all the blood flowed from the end near the extremities, whence it was coming, but none of it from the end toward the heart, whither it was going. But this plain experiment did not satisfy his opponents, who made to it the objection which we have been considering; and Harvey replies to them in this way:—

"And that no one," he says,* "may seek shelter in asserting that these things are so when nature is disturbed and opposed, but not when she is left to herself and at liberty to act; that the same things do not come to pass in morbid and unusual states as in the healthy and natural condition; they are to be met by saying that if it were so, if it happened that so much blood was lost from the further orifice of a divided vein because nature was disturbed, still that the incision does not close the nearer orifice, from which nothing either escapes or can be expressed, whether nature be disturbed or not."

It was Harvey, therefore, who appreciated fairly the influence of the disturbing causes in this case; while those who distrusted him were reduced to the preposterous assumption that cutting a vein toward the heart prevented the blood from flowing out of it, and that cutting it toward the extremities produced a current which did not previously exist. The physiologist, therefore, was right, and his opponents were wrong.

We must recollect, also, that mechanical contrivances and operations are an absolute necessity in physiological experiment. If the functions and processes of the living body were plainly exposed to view and capable of being learned by simple inspection, there would be no need of experiment of any kind. It is because they are, in fact, concealed from observation, and carried on in the secret recesses and cavities of the body, that we are obliged to resort to experiment for their investigation. Even for the most palpable and external of the vital phenomena, as, for example, the inhalation and exhalation of gases by the breath, some artificial contrivance is necessary in order to secure accuracy in our results. These artificial contrivances, accordingly, when properly used, so far from being a source of deception for the physio-

* Works of Harvey. Sydenham Edition. London, 1847, p. 121.

logist, are the necessary instruments by which he is enabled to accomplish his ends.

There are other difficulties, however, beside that just mentioned in the way of physiological investigation; and they require to be met by certain plain and simple practical rules, which are perfectly well known to physiologists, and universally adopted by judicious experimenters. Physiological experiment in fact is an art, which is to be learned, like other arts, and practised with care and judgment in order to be successful. Let me mention some of these difficulties, and the means by which they are avoided.

I. In the first place, the principal peculiarity which distinguishes the phenomena of organic life is that they are *complicated*. The result of a physiological experiment is often not a simple one, as in physical and chemical investigations, but depends on the combined operation of various forces, each of which exerts its own share of influence. The same phenomenon, therefore, is susceptible of various interpretations, until we have mastered all the elements which combine for its production. If the pneumogastric nerves, for example, which are distributed to the lungs, are cut off in the middle of the neck, this operation after some days stops respiration and produces a peculiar engorgement and solidification of the lungs, by which they are rendered incapable of supporting life. But this is not owing to the direct influence of these nerves upon the lungs; for they also send branches to other organs, and especially to the larynx, by the recurrent or inferior laryngeal nerves. The natural movements of the larynx in respiration are necessary to the healthy action of the lungs. The substance of the lung, accordingly, after division of the pneumogastrics, is solidified and engorged, not because its own nerve has been cut off, but because the branch distributed to the larynx has been cut off at the same time. The proof of this is, that if the recurrent laryngeal nerve be divided on both sides, the pneumogastrics remaining uninjured, the lungs after a time present the same kind of engorgement and solidification as in the former case.

A similar relation of this nerve and its branches was recognised by Galen, while studying the influence of the pneumogastrics in the formation of the voice.

"A lesion, accordingly," as Galen says,* "of these latter nerves destroys the voice, because the special nerves of the throat, which I call the 'recurrent nerves,' form a part of their substance. And though the former are distributed to many different parts, the latter, which belong especially to the vocal organs, have a particular claim to be regarded as the vocal nerves."

II. In the second place, the vital phenomena are *variable* at different times. The living body is not an inert structure, composed of unchangeable materials. It is an active and impressible organ-

* Galen, De Locis Affectis. Book I., Chap. VI.

ization, susceptible to the influence of external causes, and subject also to variations of an internal and periodical nature. It is most necessary, therefore, in performing a series of experiments for any purpose, that the surrounding conditions should be always absolutely identical. The quantity of material used, the quality of the instruments, the time and order in which the different steps of the operation are performed, should all be arranged with precision; for any of these circumstances are liable to influence the result. If a bottle of venous blood be drawn from the ox, and shaken up with atmospheric air, it immediately changes color, and assumes a bright arterial hue. But if another specimen of blood be drawn from the same animal in the same way, and allowed to remain at rest for forty-eight hours, and then shaken up as before, but little or no change in color takes place; for the blood, after being drawn from the vessels, suffers an alteration which renders it less susceptible to the reddening influence of the air.

The condition of the animal as to rest and activity, youth or age, fasting or digesting, will exert a similar influence. Blood drawn from the portal vein, while fasting, will present a clear and transparent serum. If drawn from the same vessel during digestion, its serum will be turbid and milky from the absorption of chyle. The blood of the renal veins, while the kidneys are at rest, is dark colored like the average of venous blood. When these organs are in active secretion, it is bright and florid, like arterial blood, and circulates with greater rapidity than before. Consequently, the blood of the portal vein and that of the jugular vein, if examined in the same animal while fasting, will be alike; if examined during digestion, they will be different. If the blood of the renal veins be examined while the kidneys are at rest, it will be venous in color; if examined while they are active, it will be arterial.

These variations, therefore, in the physiological phenomena, are not productive of deception when properly examined; but on the contrary, are the sources of greater knowledge, and lead to further discoveries. All the natural phenomena are variable to a certain extent, but they vary according to certain definite laws, which may be discovered and understood by proper investigation.

It follows from this that any apparent contradiction in physiological experiment, instead of destroying the value of its results, may in reality enhance our knowledge, and open new paths of inquiry. This has very often happened in physiology, and has been productive of the most valuable discoveries. It would therefore be a most injudicious thing for the experimenter to neglect or throw aside any phenomena which he may notice, because they appear to be in opposition to others previously observed. This would be entirely wrong; for, provided both sets of observations be carefully made, the last is as important as the first. To neglect a fact actually observed is as much a falsehood in science as to imagine one that has never been seen. Neither the new

nor the old observation, accordingly, is to be disregarded, but equal value should be given to both. If there be any apparent discrepancy between them, they will certainly be reconciled by subsequent investigation.

III. Thirdly, in experimenting upon living animals, there is a certain caution to be observed in comparing the results of the same experiment in *animals of different species.* This is very important; first of all, because different animals vary in their mode of life, in the details of anatomical structure, and in the activity of special functions,—and also because they all differ, more or less, from the human species, whose physiology is the particular object of our study. Now the rule to be adopted for our guidance in this respect, is as follows: Every experiment on the living body has usually two different results, viz. a *direct* result, and an *indirect* result; the direct result being due to its effect on the particular organ interested, while the indirect result follows, in a secondary manner, upon the disturbance caused among several associate organs, and is due to the relation existing between them.

Thus, the division of the spinal cord causes immediately paralysis of motion and sensibility in the parts below. The paralysis of the bladder also causes retention of the urine, consequent decomposition of the urea into carbonate of ammonia, an irritating condition of this fluid, and so, lastly, inflammation of the urinary passages. Paralysis of the lower extremities is therefore a direct effect of division of the spinal cord;—inflammation of the bladder is its indirect effect.

Now it is found that in all animals having the same general structure, the direct effect of a physiological experiment is the same, or varies only in rapidity, intensity, and duration. It is therefore always safe to draw conclusions from the resemblance of these direct effects in different species. The whole usefulness of physiological experiments depends upon the fact that the general structure and functions of the animals employed for that purpose are the same. In all the higher animals there is the same general arrangement of the skeleton, the same structure of the spinal cord, a similar distribution of the nerves; and the same principal organs of circulation, respiration, and digestion. The only difference between them is in the relative size and proportion of different parts, but not in their essential character. Their functions accordingly are identical. In all these animals the anterior roots of the spinal nerves are motor, and the posterior roots sensitive. In all, the lungs absorb oxygen and exhale carbonic acid. In all, the stomach secretes an acid gastric juice which dissolves the food. In all, the liver produces sugar, by the process of nutrition and secretion.

But when we come to observe the indirect results of an experiment, we find that it varies essentially in different animals, owing to the variable importance of different parts. Thus in all animals,

division of the spinal cord will produce paralysis of the hinder extremities; but it will only produce inflammation of the bladder in those animals in which the urine contains urea. Division of the seventh pair of nerves will paralyze the muscles of the face in all species; but in the horse it also produces a serious impediment to respiration, because this animal can only breathe through the nostrils, and the muscles of the nostrils are therefore more important in him than in other species. The direct effect of an operation, accordingly, which is always the same, is generally, also, the most important for our purposes; the indirect effect, which varies in different species, is often useful in explaining the peculiarities of a particular function.

It is evident, therefore, that we must exercise a certain reserve in applying the results of an experiment on the lower animals to man, or to animals of different species. If these results be direct and immediate in the mode of their production, it is always safe to make application of them to the human subject; if they be indirect, we cannot do so with certainty until we have learned all the successive steps by which they are produced.

IV. Finally, it is very essential for the experimental physiologist to be always acquainted with the *individual peculiarities* of the animals upon which he operates; and for this purpose he should keep them under his observation for a certain time before performing an experiment. For particular animals are distinguished by individual peculiarities, to a certain degree, like the members of the human species. These peculiarities relate not only to the excitability of the nervous system, the degree of intelligence and the mental disposition, but they affect the entire organization in its functions of nutrition, secretion, muscular contractility, etc. One animal will habitually feed with appetite and relish, devouring a large quantity and digesting it readily. Another takes his food sparingly, is fastidious as to its quality, and requires a much smaller quantity than the first, though to all appearance equally well nourished. In one animal the secretions are either more abundant or more easily excited than in another, the senses more acute, the motions more varied and vigorous. It is particularly in experiments upon the nervous system, in which the effect of an operation is indicated so much by the state of the disposition, the posture, manner of using the limbs, etc., that previous observation of the individual habits of the animal is requisite; for where the effect of an experiment is to be seen in the increase or diminution of a natural function, in order to appreciate these changes we must have a proper standard of comparison in the ordinary habits of the animal.

I have thus endeavored to show what experimentation upon living animals is, and in what manner it is successfully practised. Let us now see what it has accomplished.

This kind of experiment is by no means a new thing. It has been resorted to, at various times and by different observers, for

over seventeen centuries; and the periods at which medical science has especially advanced have been exactly those periods in which it has been most assiduously employed. Its results, both direct and indirect, have been incorporated with the mass of our medical knowledge, and have become so thoroughly a part of our scientific patrimony, that the source from which they were derived is often overlooked. But, in point of fact, it is not too much to say that *every important discovery in physiology has been directly due to experiments on living animals;* and that we owe many of those in practical medicine, surgery, and hygiene, either directly or indirectly to the same source.

Let us enumerate some of these discoveries, and recall the manner in which they have been made.

I. First, the *circulation of the blood.* From the time of Hippocrates down to the middle of the second century, it was supposed that only the veins contained blood, while the arteries were thought to be tubes for the conveyance of air. This was a very natural conclusion from such examinations as were made at that time. For those who opened the bodies of men or animals after death found the veins full of blood, while the elastic arteries, having emptied themselves into the veins and capillaries, resumed their cylindrical form as soon as they were cut across and the air admitted to their interior; so that the anatomist, seeing the open and gaping mouths of these vessels, concluded that they were air tubes, and called them "arteries" on that account. There were then, as every one supposed, two kinds of vessels distributed throughout the body, viz. the veins for conveying blood, and the arteries for conveying air.

This error in regard to the arteries was detected by Galen, who was accustomed to experiment upon animals, about the year 150. In the first place, he opened the arteries while the blood was still in motion, and showed that, however small the orifice, it was only blood that escaped, and not air. This, however, did not satisfy his opponents, who maintained that, in reality, air escaped first from the wound, only that it was invisible, and that the blood, which was also discharged, was drawn into the artery from distant parts after the vessel had emptied itself of air. To meet this objection, Galen exposed an artery and included a portion of it between two ligatures. Now, if the vessel were opened between the ligatures, and if it in reality contained only air, nothing but air could escape from it, as no blood could then be drawn into it from distant parts. Galen opened the vessel and found that blood escaped as before.

"For we have often," he says,* "exposed the large arteries convenient for this purpose, and asked the disciples of Erasistratus whether the artery thus exposed did not seem to contain blood.

* Galeni Opera. An Sanguis in Arteriis Natura Contineatur.

They were obliged to confess that it did, both because Erasistratus asserted that the blood passed into the arteries when they were uncovered, and because the fact was evident to the senses; for, having placed ligatures on both ends of the inclosed portion of the artery, and made an incision into the vessel between them, I showed that the artery was itself full of blood."

This was the first great discovery made in regard to the circulation, viz. that the arteries, instead of serving to distribute air, are in reality filled with blood, which moves through them to its destination.

But the remainder of the subject was still involved in obscurity. The ancients were then acquainted with only two main facts in this respect, viz. the introduction of air into the lungs, and the movement of the blood in the vessels; but how these functions were accomplished, and what was their mutual connexion, was almost unknown. Their idea of the circulatory system, as improved by Galen, was this: The mesenteric veins, coming from the intestine to the liver, brought to that organ the chyle, to be worked up in its interior into the materials of the blood. The blood thus formed in the liver was thence conveyed to the right side of the heart. After its arrival, a part of it was supposed to be filtered through small openings in the septum of the ventricles, and so conveyed into the left cavities of the organ. The blood which remained in the right cavities was venous blood, and was immediately distributed, by the veins, throughout the body to the different organs and tissues. That portion which had passed through the septum of the heart was elaborated, in the left ventricle, into a new and more active kind of blood, called "*spirituous*" blood,—the same which we now know by the name of "arterial." So the left ventricle was regarded as an organ for the elaboration of "spirits," as the liver was supposed to be an organ for the elaboration of the blood; and by a mixture of the two was produced "spirituous" or arterial blood, first fully formed in the left ventricle.

Here, then, were two kinds of blood, venous and arterial, contained respectively in two different sets of vessels—the veins and the arteries; but, according to Galen and his followers, both these two kinds of blood were conveyed from the heart, as a centre, outward to the organs and tissues, the venous current running in the same direction with the arterial, so that every organ was supplied, as he supposed, with two kinds of blood—venous blood by the veins, and arterial blood by the arteries.

This was the state of medical knowledge, in regard to the circulation, for more than a thousand years. It was somewhat modified, in the sixteenth century, by Vesalius and Servetus, who maintained that there were no openings in the septum of the heart, leading from one side to the other, but that this communication took place through the vessels of the lungs, so that that portion of the blood which passed from the right side of the heart

to the left must make its way through the pulmonary vessels and not through the substance of the heart itself. Still this doctrine attracted but little attention, and is acknowledged to have had little or no influence on the progress of physiological ideas.* Nothing, in fact, was changed except the place for the elaboration or spiritualization of a portion of the blood; this elaboration taking place, as Servetus said, not in the heart, but in the lungs.

The direction in which one-half the entire blood was supposed to move was still exactly wrong; and the blood circulated obstinately in the veins from within outward, in all books of anatomy and physiology of the sixteenth century. Even when Fabricius of Aquapendente discovered by dissection that the veins contained valves, opening toward the heart and shutting backward toward the extremities, neither he nor his pupils ever thought of any other use to attribute to these valves than that of preventing a too rapid downward current toward the limbs. So impressed were they with this fixed idea, that the blood passes from within outward along the veins, that, with the valves actually under their eyes, they did not see the physical obstacle thus presented to any current in an outward direction.

Notwithstanding the valves, therefore, the blood continued to run the wrong way for three-quarters of a century after the time of Vesalius and Servetus.

But early in the seventeenth century, Harvey began his investigations, and accomplished more than had been done by the accumulated labor of centuries. His is universally acknowledged to be the most brilliant discovery ever made in physiology; and it is difficult to imagine how any other single one of equal importance can be made hereafter. For his researches changed the whole aspect of one of the most essential functions of the living body, and showed how thoroughly its nature had been misconceived ever since the periods of antiquity.

From what has already been said, it would be natural to suppose that Harvey supplied the existing deficiency, and made everything right that was before wrong, by the simple discovery of the true motion of the blood in the veins, and its direction from the extremities toward the heart. This, at present, seems to be all that was wanting to clear up the previous obscurity, and, at the same time, the most palpable fact yet to be discovered. It appears to us almost the first thing with which a new investigator would necessarily commence.

But it happened quite differently. Harvey began with the motions and function of the heart; and the direction of the venous current, instead of being the fact with which he commenced, formed in reality the completion or termination of his work, in the order of time.

In point of fact, the movement of the blood in the veins was

* Milne-Edwards. Physiologie. Paris, 1858. Vol. III., p. 17.

not the only thing in which the older physiology of the circulation was at fault. For the true mechanism of the heart and blood-vessels was also entirely unknown. No one had any idea what caused the blood to move, nor why it moved in any particular direction. There was no definite knowledge of the physical forces of the circulation. Harvey was sensible of this deficiency, and accordingly he set to work to examine for himself, and to see by actual inspection what was in reality the action of the heart and blood-vessels. The account which he gives of his own attempts in this work, his difficulty in at once understanding what he saw, and the mode in which he gradually came to comprehend the appearances and action of the parts, is a masterpiece for faithful and graphic description, and for the candor and sincerity of purpose which appear in every word.

"When I first gave my mind to vivisections," he says,* "as a means of discovering the motions and uses of the heart, and sought to discover these from actual inspection and not from the writings of others, I found the task so truly arduous, so full of difficulties, that I was almost tempted to think, with Fracastorius, that the motion of the heart was only to be comprehended by God. For I could neither rightly perceive at first when the systole and when the diastole took place, nor when and where dilatation and contraction occurred, by reason of the rapidity of the motion, which in many animals is accomplished in the twinkling of an eye, coming and going like a flash of lightning; so that the systole presented itself to me now from this point, now from that; the diastole the same; and then everything was reversed, the motions occurring, as it seemed, variously and confusedly together. My mind was therefore greatly unsettled, nor did I know what I should myself conclude, nor what believe from others; and I was not surprised that Andreas Laurentius should have said that the motion of the heart was as perplexing as the flux and reflux of Euripus had appeared to Aristotle.

"At length, and by using greater and daily diligence, and collating numerous observations, I thought that I had attained to the truth, that I should extricate myself, and escape from this labyrinth, and that I had discovered, what I so much desired, both the motion and the use of the heart and arteries. Since which time I have not hesitated to expose my views upon these subjects, not only in private to my friends, but also in public, in my anatomical lectures, after the manner of the Academy of old."

Harvey, then, begins with the action of the heart; and he tries, as he says, "with greater and daily diligence," to understand what it is. He sees that the heart shows alternately, and with rapid recurrence, two different appearances, a pause and a motion, incessantly repeated one after the other. In the pause it is "soft, flaccid, and lying, as it were, at rest." In the motion it is hard,

* Harvey's Works, Sydenham Ed., p. 19.

erected, rises upward to a point, and "looks narrower and more drawn together." In the pause it is of a deep blood-red color; and in the motion it becomes paler, as if emptied. He thus concludes that its motion is one of tension and constriction, like a muscle, and that at this time it drives the blood out of it by contraction.

Now, this was the very opposite of what was before supposed; for it was then thought that the stroke or impulse of the heart happened at the time that it was dilated or filled with blood; not, as it seemed to Harvey, when it was emptied. But Harvey did not undertake this labor merely to replace one hypothesis by another, nor was it his disposition to make a bargain with probabilities. His genius led him at once, and without hesitation, to the positive decision of this point, by making an incision into the ventricle and seeing when the blood was actually expelled.

"But no one," he says, "need remain in doubt of the fact, for if the ventricle be pierced, the blood will be seen to be forcibly projected outward, upon each motion or pulsation, *when the heart is tense.*"

Having settled this point, Harvey goes on to the arteries; and, comparing their dilatation and contraction with that of the heart, he sees that the two sets of motions alternate with each other—that the systole of the heart corresponds in time with the diastole of the arteries, and the systole of the arteries with the diastole of the heart; and that if an artery be punctured, the blood is driven out from it at the time when the heart contracts, and when the artery itself, therefore, is dilated.

Accordingly he concludes that the impulse, or diastole, of the arteries is due to the shock of the blood driven into them by the contraction of the heart.

He gives his account of the motion of the blood through this part of the vascular system, in a manner equally striking and artistic, and with the most admirable faithfulness of detail. "Even so," he says, "does it come to pass with the motions and action of the heart, which constitute a kind of deglutition, a transfusion of the blood from the veins to the arteries." "And if any one," he continues, "bearing these things in mind, will carefully watch the motions of the heart, he will perceive not only all the particulars I have mentioned, viz. the heart becoming erect, and making a continuous motion with its auricles, but farther a certain obscure undulation and lateral inclination in the direction of the axis of the right ventricle, the organ twisting itself slightly in performing its work. And, indeed, every one may see, when a horse drinks, that the water is drawn in and transmitted to the stomach, at each movement of the throat; the motion being accompanied by a sound, and yielding a pulse, both to the ear and touch; in the same way it is with each motion of the heart, when there is the delivery of a quantity of blood from the veins to the arteries, that a pulse takes place and can be heard within the chest."

Now, at the present day, the comparison of the arterial current with the deglutition of water, through the gullet of a horse, seems a coarse illustration of a familiar and delicate function; but when employed by Harvey, it was the bold and forcible expression of an entirely unknown truth. For at that time, though the course of the blood through the arteries was known, the pulsation, both of these vessels and of the heart, was altogether misunderstood. The heart and arteries were thought to dilate simultaneously, by an expansion, somewhat like that of the chest in respiration; and the object of this motion was regarded, in a vague sort of way, as a kind of fanning or refrigeration of the blood. The action of the heart, as an *impulsive* organ, was entirely unknown, and the ventricle was considered, by the older anatomists, only as a place for the production and elaboration of what they called "spirits," and "spirituous blood." If Harvey, therefore, had done no more than this, he would still have taken rank as one of the most eminent of medical discoverers.

But this was simply the beginning of his investigations, and an introduction to his main discovery.

The venous blood was still thought to be distinct from the arterial blood. It passed, from the vena cava and right side of the heart, round through the lungs to the left ventricle; and from the left ventricle was driven through the arterial system, to the various parts of the body. But that was all. What became of it subsequently was unknown, until Harvey, led onward by his first investigations, began to think, as he expresses it, "whether there might not be a motion of the blood, as it were, in a circle," the whole blood passing from the arteries to the veins in the extremities, and again from the veins to the arteries in the cavity of the chest.

Now it is curious to observe in what way Harvey was at last led to comprehend this truth, and to convince himself of its reality. *It was by estimating the quantity of blood passing through the heart in a given time.* Harvey saw that a considerable quantity of blood was driven out of the heart at each pulsation. Estimating this quantity at its lowest possible amount,—half an ounce, three drachms, or even one drachm, according to the size of the heart,—he was struck with the remarkable fact that, in the course of half an hour or an hour, more blood passed through the heart, from the veins to the arteries, than was contained in the whole body. As this passage was incessantly going on with the same rapidity, it was obvious that the necessary quantity of blood could not be supplied, as Galen had supposed, from the food and drink, since it would require at least forty or fifty times as much food as is taken in the whole course of twenty-four hours. Harvey therefore came to the conclusion that the same blood which passes outward in the arteries is brought back, by a returning current, in the veins. Finally, he came to the positive demonstration of this returning current, by opening the veins,* and

* Harvey's Works, Sydenham Edition, pp. 50, 53, 65, 120, 141.

tying and compressing them at various points;—and thus saw and proved that the blood returns to the heart by the veins, and so, following a continuous round, performs the incessant movement of the circulation.

Such was the history of the discovery of the circulation. Let us now pass to that of *Respiration.*

II. The first important acquisition in regard to respiration was made by Sir Robert Boyle, in 1670.* He employed for this purpose the air-pump, then recently invented, by which he was enabled to exhaust the air from a closed receiver. He experimented on kittens, birds, frogs, fish, snakes, and insects; and showed that in all these animals the presence of atmospheric air is necessary to the maintenance of life, and that when stupefied by its withdrawal they may be resuscitated by reädmitting it. This was confirmed, in a more positive manner, in regard to fish, by the Swiss experimenter, Bernouilli,† who found that ordinary water contains atmospheric air in solution, and that when deprived of this by boiling, it becomes unfit for the respiration of fish. In this way it was established that in all species, both terrestrial and aquatic, it is the atmospheric air which is essential to respiration.

The next important fact discovered was that atmospheric air, by continued breathing, becomes vitiated and unfit for respiration. This was shown first by Boyle, who, by confining animals in a closed vessel, found that after a time they became suffocated, and that, in order to keep up respiration, it was necessary to remove the vitiated air and introduce a fresh supply.

The third point was established by Mayow,‡ in 1674, who, by experimenting upon mice, found that by their respiration the air was not only vitiated, but also diminished in volume. He measured the amount of this diminution, and found that it was seven per cent. of the whole volume of the air. He concluded that something in the air was used up or consumed by respiration, and he gave to this substance the name of "nitro-aëreal spirits."

Fourth, Priestley, in 1772,§ continued these experiments on air vitiated by respiration, in order to determine how it might be regenerated and again made capable of supporting life. He inclosed mice in tight receivers until the air was thoroughly vitiated and rendered unfit for respiration. He then subjected this air to a great variety of processes, and again tested it by introducing into it other mice. He tried, in this way, the effect of long keeping, of the application of a high temperature, condensation, rarefaction, and contact with vegetable mould; all without success. Finally, he found that by making plants grow in it the vitiated

* Boyle. Philosophical Transactions, Vol. V., pp. 2011-2055.
† Milne-Edwards. Physiologie. Paris, 1857, Vol. I., p. 382.
‡ Ibid., p. 389.
§ Philosophical Transactions, Vol. LXII., p. 147.

air became regenerated, and again fit for the respiration of animals. He thus established the reciprocal relation of animal and vegetable respiration; viz. that animals, by breathing, deprive the atmospheric air of a certain property which is restored to it by vegetation.

Black had already, a few years before, studied the properties of carbonic acid—which he called "fixed air," because it is fixed in the calcareous and alkaline carbonates—and found it to be a product of the respiration of men and animals.

But the greatest advance in our knowledge of this function was that made by Lavoisier, from 1775 to 1780;—when he established, at almost the same time, the true composition of the atmospheric air and the part which it plays in respiration. His experiments, so far as they relate to our present subject, may be briefly stated as follows. He deoxidized atmospheric air by the calcination of mercury, and found: First, that animals could no longer breathe in it; and secondly, that it had diminished in volume. He concluded that the atmosphere was composed of two airs or gases; one respirable, the other non-respirable; that the respirable air was that which had combined with the mercury (oxygen), and the non-respirable that which was left behind. He then collected the oxide of mercury, reduced it again to the metallic state, collected the gas which was evolved in this process, and found that it amounted to exactly the same volume by which the air had been previously diminished. He then added this evolved gas to the non-respirable residue, and found that the mixture had again become respirable, and that animals no longer died in it. These experiments were performed with sparrows.

He then went further, and examined more closely the air which had been vitiated by continued respiration. He saw that, in its incapacity to sustain life, this vitiated air resembled that which had been simply deoxidized by the calcination of mercury. But, on further examination, he found two points of difference between these cases, viz. First, that the diminution of volume was much less in one case than in the other; and secondly, that the air vitiated by respiration precipitated lime-water and consequently contained carbonic acid, while the other did not. He found also, that, in order to renovate the air which had been vitiated by respiration, two things must be done. Not only we must take away from it, by means of lime or a caustic alkali, the carbonic acid which it contains, but we must also restore to it a quantity of oxygen equal to that which it has lost. He established in this way the following important conclusions:

First, that respiration acts only on the respirable portion of the air, or oxygen, while the remainder, or nitrogen as we call it, is entirely passive in the process; and Secondly, that when animals are confined in a limited space, they die when they have absorbed, or converted into carbonic acid, the greater part of the oxygen,

and so reduced the atmosphere to the state of an irrespirable or mephitic gas.*

In this manner were developed the two main features in the process of respiration, viz. absorption of oxygen and exhalation of carbonic acid. Other details of interest and value have since been added by other observers, relating to the time, place, quantity, and manner of the production and disappearance of these two gases. But the principal facts were established at that time; and our real knowledge of the function of respiration dates from Lavoisier, as that of the circulation dates from Harvey.]

There are two operations immediately dependent on the functions of circulation and respiration, the history of which is directly connected with our present subject.

III. One is that of *Transfusion of the Blood* The earliest form in which the idea of transfusion presented itself was that of injecting into the blood-vessels certain medicinal agents. This was first done in England about the middle of the seventeenth century, under the auspices of Sir Robert Boyle; at which time it was shown that a solution of opium might be injected into the blood-vessels of a dog, so as to produce a narcotic effect upon the brain without killing the animal.

After the feasibility of this operation had been established, it excited much interest in various parts of Europe. It seems even that the priority of the discovery was a matter of some dispute; for there appeared in the Philosophical Transactions of the Royal Society of London, for 1665, the following communication, entitled,

"*An Account of the Rise and Attempts of a Way to conveigh Liquors immediately into the Mass of Blood.*

"Whereas, There have lately appeared in publick some *Books* printed beyond the Seas, treating of the Way of *injecting Liquors into Veines;* in which Books the *Original* of that *Invention* seems to be adscribed to others besides him to whom it really belongs: It will surely not be thought amiss if something be said whereby the true *Inventor's* right may beyond exception be asserted and preserved: To which end there will need no more than barely to represent the *Time* when and the *Place* where and among whom it was first started and put to tryal. To joyn all these circumstances together 'Tis notorious that at least six years since (a good while before it was heard off that any one did pretend to so much as thought of it) the Learned and Ingenious *Dr. Christopher Wren* did propose, in the *University of Oxford* (where he is now the worthy Savilian Professor of *Astronomy*, and where very many curious Persons are ready to attest this Relation) to that noble Benefactor to Experimental Philosophy, *Mr. Robert*

* Lavoisier. Expériences sur la Respiration des Animaux. Mémoires de l'Académie des Sciences Paris: 1777.

Boyle, Dr. Wilkins and other deserving Persons, that he could easily contrive a way to conveigh any liquid thing immediately into the Mass of Blood; videl.: by making ligatures on the Veines, then opening them on the side of the ligature towards the heart, and by putting into them slender Syringes or Quills, fastened to Bladders (in the manner of Clyster-pipes) containing the matter to be injected; performing that operation upon pretty big and lean Doggs, that the vessels might be large enough and easily accessible.

"This proposition being made, Mr. Boyle soon gave order for an apparatus to put it to experiment; wherein at several times, upon several doggs, *Opium* and the infusion of *Crocus Metallorum* were injected into that part of the hind legs of those animals, whence the larger vessels that carry the blood are most easy to be taken hold of: whereof the success was that the opium, being soon circulated into the brain, did within a short time stupefy tho' not kill the dog."*

These researches were soon followed by those of Richard Lower, on the transfusion of blood from the vessels of one animal into those of another. His first experiments were done in 1665, and were reported by Mr. Boyle to the Royal Society in the following year. The experiments consisted in placing a ligature upon a dog's carotid artery, opening the vessel below the ligature, and inserting into it a quill of the proper size to serve for a canula. This canula was then connected with the jugular vein of another dog, so placed as to receive the blood coming from the carotid artery of the first in a direction to pass downward to the heart and so mingle with the mass of the circulating current. The jugular vein of this dog was then opened above the insertion of the canula, and he was allowed to bleed from it; the blood lost in this way by the second dog being replaced by that which was transfused from the vessels of the first.

These experiments were quite successful, and first showed that death from hæmorrhage might be prevented by transfusion. Lower, however, found one practical difficulty in the performance of the experiment, viz. that the blood of the first dog sometimes coagulated in the artificial tube and stopped the current; so that he was obliged to take out the canula and clear it with a probe.

"For this must be expected," he says,† "when the dog that bleeds into the other hath lost much bloud, his heart will beat very faintly, and then, the impulse of bloud being weaker, it will be apt to congeal the sooner. . . . But to prevent this trouble and make the experiment certain, you must bleed a great dog into a little one, or a *Mastive* into a *Curr*, as I once try'd, and the little dog bled out at least double the quantity of his own bloud, and left the Mastive dead upon the table, and after he was

* Philosophical Transactions. Vol. I., p. 128.
† Philosophical Transactions. Vol. I., p. 353.

untyed, he ran away and shaked himself, as if he had only been thrown into water."

These results soon led to the idea of performing the same operation on the human subject. This was first done in France by a physician and a surgeon, named Denis and Emmerets. They were led to believe that the operation might result in the cure of diseases by introducing healthy blood from a foreign source into the veins of the patient; and some apparently successful results of this kind excited among the profession great enthusiasm in its favor. But these expectations soon proved unfounded, and several cases of failure afterwards produced so much reaction against it, that in 1668 the Parliament of Paris passed a law forbidding the operation to be performed except by consent of the Faculty.

The matter remained in this condition until 1818, when Dr. James Blundell, of London, brought back the operation to its original object; applying it, not to the cure of diseases, but to the preservation of life after exhausting hæmorrhage. He also discovered and exposed the principal error in the previous mode of doing the operation; viz. the use of blood of animals of a different kind, instead of those belonging to the same species.

He performed thirty-three experiments on animals, and showed by them:

1st. That dogs exhausted by hæmorrhage may be resuscitated, even after momentary stoppage of the respiration, by injecting the blood of other dogs.

2d. That human blood, injected into a dog in large quantity, sufficient to replace an exhausting hæmorrhage, though it produces a temporary reanimation, does not save life, but the animal dies some hours afterwards.

3d. That transfusion of blood, in animals of the same species, will be successful, whether the blood used be arterial or venous.

4th. That blood may be received into a cup and passed through a syringe, without being thereby rendered unfit for the purposes of life.*

The transfusion of blood, thus placed upon its proper footing, and still further improved by the investigations of Prevost and Dumas, Milne Edwards, Dieffenbach, and Bischoff, was adopted by the profession, and is now known as an established and useful operation, applicable to cases in which an exhausting hæmorrhage, in a healthy person, has brought the patient to the point of death. In these cases it has long been known, as mentioned by Blundell, that there is an interval, often of several hours, after the hæmorrhage has ceased, during which the patient is evidently sinking, and when other means of restoration are of no avail. Bérard has recorded fourteen such cases, in which the operation of transfusion was successful. They are as follows:†

* Researches, Physiological and Pathological; by James Blundell, M.D., London, 1824.

† Bérard, Cours de Physiologie, Paris, 1852. Vol. III., p. 219.

1st. A case of abundant hæmorrhage following delivery. All the signs of approaching death. Extremities cold; respiration imperceptible. Two ounces of blood were transfused by Blundell, from the husband, and the operation repeated a few minutes afterwards. Recovery.

2d. By Dr. Doubleday. Uterine hæmorrhage. The patient was at the last extremity. Six ounces of blood were tranfused from the husband in three injections. The patient returned from death to life. Recovery.

3d. By Brigham, of Manchester. Bleeding after delivery. Loss of consciousness; cold extremities. Death near at hand. Ten or twelve ounces of blood were injected at several times. At the third injection the patient began to revive. Recovery.

4th. By Dr. Burton Brown. Hæmorrhage after delivery. The patient presented a cadaverous aspect. Extremities cold; pupils dilated; no pulse, either at the radial or carotid. Three injections of blood at intervals of five to ten minutes. Recovery.

5th. By Waller. Uterine hæmorrhage. Death imminent. Thirty-nine drachms of blood were injected at intervals of five minutes. Recovery.

6th. By Banner, of Liverpool. Uterine hæmorrhage. Unconsciousness. About fourteen ounces of the blood of the husband injected at six intervals. The patient's condition remained doubtful for some time, but terminated in recovery.

7th. By Ingleby. Uterine hæmorrhage after delivery. Patient cold and insensible. Injection of four ounces of blood furnished by the husband. After seven minutes the pulse reappeared. After thirty minutes the patient was partially restored. Ultimate recovery.

8th. By Dr. Klett. Non-puerperal uterine hæmorrhage. Cold sweat, dimness of vision, hiccup, and an altered expression of face, announcing the approach of death. Injection of blood furnished by the husband. The patient at once opened her eyes, and her countenance changed for the better. Recovery.

9th. By Dr. Klett. Uterine hæmorrhage. The patient, whose aspect was cadaverous, took leave of her friends. Articulation imperfect. A small quantity of blood was injected from the husband. Life was reëstablished as if by the effect of electricity.

10th. By Mr. Lane. A young man subjected to the operation for strabismus. Obstinate hæmorrhage. Condition of the patient desperate. Three and a half ounces of blood injected from a young woman. Recovery.

11th. By M. Bougard. A female patient, exhausted by hemoptysis and subsequent abundant hæmorrhages. Injection of several ounces of blood, furnished by a young woman. The patient lived till the following year.

12th. By Dr. Savy. Uterine hæmorrhage in the third month of pregnancy. Skin icy cold; eyes lustreless; lips pale; limbs relaxed. Transfusion of blood, furnished by a healthy servant. Recovery.

13th. By Nélaton. In this case the result was finally unsuccessful, as the patient died of puerperal complications; but the first effect of the operation was as satisfactory as possible.

14th. By Domène. Hæmorrhage after delivery. Signs of approaching death. Transfusion of blood from a woman. Consciousness returned a few minutes after the operation. Recovery.

Two successful cases of transfusion are also recorded in the British and Foreign Medico-Chirurgical Review for July, 1857: one by the late Professor Brainard, of Chicago, in the *Chicago Medical Journal* for February, 1860; and one in the *New York Medical Journal* for November of the present year.

In these cases, as noticed by Bérard, there is no doubt that the operation saves the life of the patient; for in animals reduced by hæmorrhage to the condition of apparent death, recovery never takes place unless transfusion be performed.*

IV. Next, *Artificial Respiration.* There can be no need to do more than allude to the usefulness of this operation, which has so often been successful in cases of suspended animation. It is intimately associated with the nature and mechanism of natural respiration, and these two elements of the function were naturally studied, to a great extent, together. In the middle of the sixteenth century Vesalius had found that by blowing up the lungs with air, after the chest was opened, the stoppage of the heart's action might be delayed for a short time. But the first attempt at continuing life indefinitely in this way was that of Robert Hook in 1664, who showed that by alternately inflating the lungs with bellows, and then allowing them to collapse, an artificial respiration might be established which could be kept up for a long period. Hook reported this result to the Royal Society, where it attracted all the attention which its importance deserved.

"I did, therefore," he says, in a second communication,† "heretofore give this *Illustrious Society* an account of an Experiment I formerly tryed of keeping a Dog alive after his *Thorax* was all displayed by the cutting away of the *Ribs* and *Diaphragme*, and after the *Pericardium* of the Heart was also taken off. But divers persons seeming to doubt of the certainty of the Experiment (by reason that some Tryals of this matter, made by some other hands, failed of success), I caused at the last Meeting the same Experiment to be shown in the presence of this *Noble Company*, and that with the same success as it had been made by me at first."

But this was not sufficient. Plain as the matter seems to us now, it was not then certainly known in what manner the continuance of respiration maintained the circulation of the blood; and by some this was thought to be accomplished not through the

* Cours de Physiologie. Vol. III., p. 222.
† Philosophical Transactions. Vol. II., p. 539.

chemical influence of the atmospheric air, but by the physical movement of the parts. To settle this point Hook contrived another experiment. Having opened the chest as before, and fitted his bellows to the trachea, he then fitted a second pair of bellows to the further end of the first, and at the same time made numerous incisions over the outer surface of the lungs. He then worked the second pair of bellows very rapidly, "by which," he says, "the first pair was always kept full and always blowing into the lungs," the lungs being themselves always full and without motion, but having a constant blast of air passing through them. Life was sustained in this experiment as in the other. Hook therefore showed,

1st. That life might be indefinitely prolonged by the alternate inflation and collapse of the lungs in artificial respiration; and

2d. That this was accomplished, not by the simple motion of the parts, but by the continual introduction of fresh air into the respiratory passages.

Artificial respiration was subsequently employed by Brodie, Hope, Legallois, Wilson Philip, and others, both for studying the action of the heart and blood-vessels, and for resuscitating asphyxiated animals. It was also applied to the human subject, and is now a recognised means of preserving life in cases of drowning, hanging, asphyxia from breathing noxious gases, and the suspended animation of the newly born.

V. *Digestion.*—There is a curious fact in the physiological history of digestion, viz. that the first source of definite knowledge on this subject consisted of experiments performed, not upon animals, but upon a man. Alexis St. Martin, who was accidentally shot in 1822, in such a way as to produce a gastric fistula, enabled Dr. Beaumont to make a series of observations which were attended with valuable results. Dr. Beaumont then positively demonstrated:

1st. That the stomach secretes a peculiar acid fluid, which is known as the gastric juice.

2d. That the gastric juice is secreted only during digestion and when the stomach is excited; and

3d. That it has the power of dissolving the ingredients of the food, and is accordingly a true digestive secretion.

But we cannot always have a man with a gastric fistula to experiment with; still less make one in the human subject at will. Accordingly the subject has since been studied by means of fistulæ artificially established on animals. I need not recall all the discoveries which have been made in this way by Blondlot, Schwann, Bernard, Lehmann, and others. I would only mention two important considerations: First, that these subsequent labors have fully confirmed the principal facts discovered by Beaumont, and have shown that the properties of the gastric juice are essentially the same in man and animals; and, secondly, that by them

our knowledge of the subject has been greatly extended, and some of Dr. Beaumont's errors—unavoidable for a first experimenter in a new field—have been explained and corrected. Our knowledge of the remainder of the digestive fluids, the pancreatic and intestinal juices, and the bile, as well as the sugar-producing function of the liver, has been almost entirely due to experiments on the lower animals.

VI. *The Nervous System.*—The study of the nervous system seems to have been more difficult than that of the other great divisions of the animal organism. This is probably because the functions of the nerves and nervous centres are so different in their nature from the physical and chemical phenomena of nutrition, that they require, in most instances, peculiar methods of investigation, not adapted to the study of the latter. It is also, no doubt, partially owing to the fact that the intricate structure of the nervous system requires a longer study of its anatomy before that of its functions can be successfully pursued. However this may be, it is certain that the more important discoveries in this department date from a later period than the others. Galen discovered, by experiments upon living animals, the functions of the recurrent laryngeal nerves, as connected with the voice. But from his time the labors of investigators were principally confined to unravelling the anatomy of the nervous system, until the early part of the present century. The first important advance was made at that time by the discovery that the endowments of sensation and motion occupy distinct localities in the organs of the nervous system.

The exact history of this discovery has been open to some dispute, owing mostly to the fact that the ideas of physiologists on this point were developed somewhat slowly, and only gradually assumed the definite and precise form which they now present. The main part of the discovery has been claimed principally for two experimenters, viz. Sir Charles Bell in England and Magendie in France. The share which each really had in accomplishing this result is as follows:

Sir Charles Bell's investigations were guided by an idea, based upon anatomy, that the different nervous tracts, connected with different parts of the brain, had different endowments. He considered the cerebrum as connected with the anterior columns of the spinal cord and the anterior roots of the spinal nerves, and the cerebellum as connected with the posterior columns and the posterior roots. His first experiments, in 1811, on the spinal cord and nerves, were undertaken for the purpose of ascertaining, through them, the distinct endowments of the cerebrum and cerebellum.*

* Bell. Nervous System of the Human Body, 3d edition, London, 1844; p. 24; also, "An Idea of a New Anatomy of the Brain;" quoted in a Narrative of the Discoveries of Sir Charles Bell in the Nervous System, by Alexander Shaw. London, 1839; p. 40.

These experiments consisted in laying open the spinal canal of the rabbit and irritating alternately the different columns of the cord and the different roots of the nerves. He found by this means that irritation of the anterior columns and roots produced convulsion of the muscles; while irritation of the posterior columns and roots had no such effect.* These experiments were performed, first, upon the uninjured animal, and subsequently upon those which had been stunned by a blow on the head.† They showed simply that the power of motion resided in the anterior roots of the spinal nerves, but not in the posterior roots; and consequently that the nervous roots, as well as the columns of the cord, were dissimilar to each other. It did not then appear where the power of sensation was located; nor what was in reality the function of the posterior roots.‡ This was necessarily left in doubt; since, in all but the first of these experiments, sensibility had been destroyed before opening the spinal canal.

The next and most successful experiments of Sir Charles Bell were those performed, in the living animal, on the nerves of the face. They were communicated to the Royal Society of London in 1821.§ These were certainly the first experiments which showed the important fact that the Seventh cranial nerve is a nerve of motion, while the sensibility of the parts resides in the Fifth pair. But even here, Bell's idea was not that of the simple distinction of sensation and motion in these two nerves. His object was to show the existence of a great system of *Respiratory Nerves*, separate from those of sensibility and voluntary motion. He regarded the Seventh pair as the great Respiratory nerve of the face, and proved that when it is divided in the living animal, the respiratory movements of the lips, nostrils, and cheeks are abolished, while sensation remains. But he still regarded the Fifth pair as a nerve of sensation and voluntary motion for the whole face; the Seventh being simply superadded, to provide for the movements connected with respiration. It is impossible to read his original paper, in the Philosophical Transactions for 1821, without being convinced of this fact.

Nevertheless, this discovery was a very important one, and led to important practical results. For, before that time, surgeons were in the habit of cutting the Seventh nerve for the cure of facial neuralgia—of course, with very unfavorable results, as the operation not only failed to relieve the neuralgia, but also produced a paralysis of motion in the face. But after Sir Charles Bell had shown that the Seventh was only a nerve of motion, while sensibility resided in the Fifth pair, the old operation was abandoned, and accordingly it is now the Fifth pair only which is divided for the relief of tic douloureux.

* Narrative of the Discoveries of Sir Charles Bell, &c., p. 41.
† Ibid. p. 87. ‡ Ibid. p. 46.
§ Philosophical Transactions, Vol. XXXI., p. 398.

In 1822 the study of the spinal nerves was taken up by Magendie, and the separate endowments of the two roots fully established. In order to exhibit the exact and valuable character of these experiments, and to show how little they deserve the violent criticism which has sometimes been made upon them, I will quote the original report by Magendie, in the *Journal de Physiologie Expérimentale et Pathologique*, Vol. II. (1822); p. 276:

"For a long time," he says, "I was desirous of doing an experiment upon an animal in which I should divide the posterior roots of the spinal nerves. I had several times made the attempt, but failed, in consequence of the difficulty of opening the spinal canal without wounding the cord, and either destroying the animal or inflicting upon him a serious injury. But in the course of the last month there came into my possession a litter of eight pups, only six weeks old; and I thought this a good opportunity of renewing my attempt to open the spinal canal. The result was that I was enabled, by the aid of a sharp cutting scalpel, to uncover, nearly at a single stroke,* the posterior half of the spinal cord, still enveloped by its membranes. The only thing then remaining to expose almost completely the surface of the cord, was to cut through the *dura mater* surrounding it. This was accomplished without difficulty, and I then had under my eyes the posterior roots of the lumbar and sacral nerves. After taking up these roots, one after the other, with the blade of a fine pair of scissors, I divided them on one side, the cord remaining untouched. I was uncertain what would be the result of this operation. Accordingly I sewed up the wound in the integuments and kept watch upon the animal. At first I thought that the limb corresponding to the divided nerves was entirely paralysed. It was insensible to pricks with a pointed instrument and to the strongest pressure, and it also appeared to be motionless. In a few moments, however, to my great surprise, I saw it move very distinctly, though sensibility had altogether disappeared in it. A second and a third experiment gave precisely the same result; and I began to regard it as probable that the posterior roots of the spinal nerves might, indeed, be different in function from the anterior roots, and that they might be more particularly devoted to sensibility.

"It very naturally occurred to me also to cut the anterior roots, leaving the posterior untouched. But such an enterprise was more easily conceived than executed; for how could the anterior part of the cord be laid bare without injuring the posterior roots of the nerves? At first, the undertaking appeared to be hopeless; but, after thinking the matter over for forty-eight hours, I determined to make the attempt to pass in front of the posterior roots, a kind of cataract-knife, with a very narrow blade, so that I might cut the nervous roots by pressing

* Owing to the cartilaginous condition of the vertebræ in young animals.

the edge of the knife against the posterior surface of the bodies of the vertebræ. I was obliged, however, to give up this plan, on account of the large veins which occupy that part of the spinal canal, and which I wounded at each attempt to carry the knife forward. But in making these trials, I noticed that, by drawing aside the dura mater, I could catch a glimpse of the anterior roots, united into bundles, just at the spot where they penetrate the membrane. This was all that I desired, and in a few seconds I had divided all the anterior roots upon which I wished to operate. As in the previous experiments, I made the division on one side only, that I might use the other as a term of comparison. It may easily be imagined, with what curiosity I watched the effects of this operation. The result was unequivocal. The limb was completely motionless and relaxed, while at the same time it retained a perfectly evident sensibility. Finally, that nothing might be neglected, I divided at once the anterior and posterior roots. This produced entire loss both of sensibility and movement.

"I have repeated these experiments in various ways, in several species of animals; and the results above described were confirmed in the most complete manner, both for the anterior and posterior limbs. I am still continuing these researches, of which I shall give a more detailed account in a succeeding number. For the present, it is sufficient that I can state positively that the anterior and posterior roots of the spinal nerves have different functions; and that the posterior appear more particularly devoted to sensibility, while the anterior are more especially connected with the power of motion."

In this way was the distinction of sensibility and motion in the spinal nerves finally established.

It is a little remarkable, in this respect, that Sir Charles Bell should be sometimes quoted as speaking in condemnation of this kind of experiment; for whenever he does so, he appears as a witness against himself. In reviewing, at the present day, the works of Sir Charles Bell and their results, it is evident that everything of value in the physiology of the nervous system which he discovered, viz. the motor character of the anterior roots, and the distinction between the fifth and seventh cranial nerves, he discovered by means of experiment upon living animals. These discoveries were positive and permanent, and retain at this day their full value. But all Sir Charles Bell's theoretical views, which he thought of so much importance, such as the division of the nerves into *regular* and *irregular* or *superadded*, and the great *respiratory system* of nerves, are now destitute of interest, and have nearly passed out of mind.

There is one circumstance connected with the discovery of the distinct properties of the spinal roots, which is so curious in some respects that it has become a kind of joke in the history of physiology. At quite an early period, in 1809,

Alexander Walker conceived the idea, on certain theoretical grounds, that the anterior spinal roots were nerves of sensation, and the posterior roots the nerves of motion. He was thoroughly convinced of the truth of this doctrine; so much so, that twenty-five years later he published a work entitled "The Nervous System, Anatomical and Physiological," in which he reiterated his views, and maintained that Sir Charles Bell and Magendie were entirely wrong. In vindicating his claims in this work, he says (page 136):

"In 1809, viz. twenty-five years ago, the writer published the true doctrine of the nervous system; *a.*, ascribing sensation and action to distinct nerves and columns; *b.*, ascribing these truly, viz. sensation to the anterior and action to the posterior nerves and columns."

Also (page 137), "In 1815, viz. six years after his first publication of this doctrine, the writer repeated and extended it, in *Thompson's Annals of Philosophy*, still ascribing sensation and action to distinct nerves and columns,—and ascribing these truly, viz. sensation to the anterior and action to the posterior nerves and columns."

Also (page 138), "Thus the title, to which either Sir Charles Bell or M. Magendie have presumed to lay claim, viz. the ascribing distinct functions to distinct nerves and columns (and ascribing these wrongly, as the writer has shown), dates twelve or thirteen years after this was done (and done rightly) by the present writer."

Now, Alexander Walker had a very bad opinion of experiments, especially when performed upon the nervous system, to which, he thinks, they are "totally inapplicable."* He has a paragraph on page 97, headed:

"*Utter Worthlessness of all Experiments on Living Animals, in which Functions are destroyed or disturbed.*"

In the course of this paragraph he remarks (page 105), "The rage for experiments on living animals will now be easily understood. All fools have eyes and fingers. They rejoice that some fantastic use of these may be called experiment. This becomes a happy compensation for the want of reasoning powers. The number of pretenders naturally renders the method universal; and each blockhead thinks himself wise when he has made an experiment which he does not understand. Henceforth," he says, "let these men know that amplitude of eye and hand is a wretched substitute for littleness of brain, and that reasoning powers are essential to all advances in physiology."

In a previous paragraph (page 11) the writer says that, where

* The Nervous System, Anatomical and Physiological. By Alexander Walker. London: 1834, p. 105.

we are dealing with living and complicated functions, "the performance of experiment is the act of a person who has not the slightest notion of the use and application of experiment, and it becomes the mere play of a driveller or an idiot. Of this," he adds, somewhat innocently, "the reader will have a striking illustration in the sequel."

From his description of certain experiments, however, it must be confessed that Walker does not seem to have had any practical acquaintance with them.

Subsequently to 1822, other discoveries of great interest in the nervous system have been made: by Longet, Magendie, and Bernard, on Recurrent Sensibility; by Mayo and Marshall Hall, on Reflex Action; by Schiff, Vulpian, and Philipeaux, on the Regeneration of Nerves; by Legallois and Flourens, on the Properties of the Medulla Oblongata; and by Brown-Séquard, on the Crossed Action of the Spinal Cord;—all by means of experiments on living animals.

VII. Experiments of this kind formed a large part of the discovery, by John Hunter, of the modern *Operation for Aneurism*. Previously to 1785, the best treatment for this affection consisted in opening the aneurism and tying the vessel in the wound, or in applying a ligature immediately above and below the part. This operation, however, was usually unsuccessful, owing to fatal secondary hæmorrhages from the aneurismal tumor, or from the ligatured part of the vessel. The manner in which the improvement in treatment took place was as follows:

"Mr. Hunter,* from considering the manner in which the spontaneous cure of external aneurisms was effected, was led to propose and practise, in the year 1785, the operation which justly bears his name. . . . The only point on which he required information was, whether the repeated hæmorrhages which took place (when the ligature was placed immediately above the aneurism) were really in consequence of the artery being diseased; and this he endeavored to elucidate by observation and experiment. He laid bare the inner coat of an artery (in the dog) by dissecting off by layers the two outer ones until he saw the blood through the remaining membrane; the wound was then closed, and the animal was killed three weeks afterward, when the parts were found consolidated, the canal of the artery being neither increased nor diminished. This was not supposed to be sufficient to establish the fact that an injury of this kind might be inflicted on a sound artery with impunity; and Sir Everard Home, after dissecting off the outer and middle coats, in a similar manner, laid a piece of lint upon the artery, to prevent adhesion, instead of closing the wound. It healed up at the end of six weeks, when the dog was killed, and the artery being injected, the coats of it were found of their natural thickness and appearance.

* Guthrie, on the Diseases and Injuries of Arteries, London, 1830, p. 148.

"Satisfied by the result of these experiments that ulceration of the artery depended more on its being previously diseased than on the injury committed upon it, he fairly supposed that if he placed his ligature higher up on the artery, on a sounder part, he would have a better chance of avoiding the hæmorrhages which had so frequently proved fatal."

Experience afterward showed this operation to be successful, and it has since been the means of saving many limbs and of preserving many lives which would have been sacrificed under the older method of treatment.

VIII. There are few improvements in surgery which have been attended with more satisfactory results than those which were introduced into the operations of resection and extirpation by the discovery of the *Office of the Periosteum in the regeneration of bone.* When a large portion of any bone requires to be removed, on account of fracture or disease, the permanent deficiency of the bony parts, after the healing of the wound, is a very serious inconvenience; so much so that surgeons were frequently in the habit of amputating the entire limb rather than retain a member which, if preserved, would be practically an incumbrance to the patient instead of an advantage. The study of the mode of reparation of bone, by means of the periosteum, has shown how this difficulty may be avoided, and how much advantage may be gained in the preservation of injured and diseased parts.

Du Hamel, in 1740,* and Hunter, in 1772,† first learned, by experiments on pigeons, fowls, and young pigs, that the growth of bone takes place mainly from the exterior, and probably by the nutritive power of the periosteum. Subsequently this question was further examined experimentally by Flourens, Heine, Murray, and others. Mr. Syme, of Edinburgh, in 1837,‡ endeavored to ascertain, as he expresses it, "whether the periosteum, or membrane that covers the surface of the bone, possesses the power of forming new osseous substance independently of any assistance from the bone itself." He extirpated the middle portion of the radius of a dog, with its periosteum, and found, as Sir Astley Cooper had previously done, that after the recovery of the animal there was no bony union of the parts, but only a ligamentous band, running from one bony extremity to the other. At the same time he did a similar operation on the radius of the opposite leg, only leaving the periosteum in its place; and on this side he found, at the end of six weeks, instead of the gap presented by the first leg, "a compact mass of bone occupying the space left by the portion removed." He also detached the periosteum in another dog without removing the bone, and inserted a piece of

* Mémoires de l'Académie Royale des Sciences, 1741, '42, and '43.

† Works of John Hunter. Palmer's edition. London, 1835. Vol. IV., p. 315.

‡ Transactions of the Royal Society of Edinburgh. Vol. XIV., p. 158.

metal under the membrane, in which, at the end of six weeks, he found a thin plate of bone, entirely disconnected with the original osseous surface.

Wagner, in a monograph on the *Process of Reparation after Resection and Extirpation of the Bones*,* points out the usefulness of these experiments, and the reasons upon which their value depends.

"The anatomical study of the process of reparation," he says,† "may be made either on man or on animals. It would be better, no doubt, to make this study upon man, but for this purpose it would be necessary to have the opportunity of dissecting, at different periods, human limbs which had been the subjects of resection. It would be necessary that the patient should die, or that secondary amputation should be performed; and, judging from the poverty of our knowledge in this respect, it seems probable that such opportunities are only rarely met with. If so, this would go to prove the safety of the operation of resection. It is true that it is still more rare to have the opportunity of dissecting the body of a person who has before been the subject of resection, and who has survived a certain time after the completion of the cure.

"We must, therefore, have recourse to experiments on animals, and by this means we may follow all the steps of the reparative process."

Wagner's experiments, which were mainly performed with dogs and pigeons, carried still further our knowledge of the process of reparation and the part taken in it by the periosteum.

But the most recent and extended investigations on this subject were those of Leopold Ollier, in 1858.‡ He operated by separating, in the living animal, a lamina of periosteum from the surface of the tibia and transplanting it among the neighboring muscles. He found that the periosteum, thus removed from its original locality, not only continued to live, but produced bony tissue in its new and unaccustomed situation. This happened even when the lamina of periosteum was entirely separated from its original attachments and transplanted, like the skin in autoplastic operations on the face. It took place even when the separated periosteum was transplanted into distant regions, as into the popliteal space, or under the skin of the groin or that of the back. In all these cases, the new bone produced had the natural structure of osseous tissue, as shown by microscopic examination.

Ollier also practised resections and extirpations of the long bones, making always a comparative experiment in order to judge of the influence of different modes of operating. He extirpated the radius and metatarsal bones, upon rabbits and dogs, taking

* In Archives Générales de Médecine (1853), Vol. II., p. 712.

† Ibid, p. 713.

‡ *Récherches Expérimentales sur la Production Artificielle des Os*, &c. Journal de la Physiologie de l'Homme et des Animaux. Janvier, 1859.

away also the periosteum in some instances, and in others leaving it undisturbed. He found that when the periosteum was left, a new bone was developed in from one to three months afterward; but when it was removed, no bony regeneration took place. In extending his observations to cases of resection of the joints, Ollier found that when the articulating extremities of the bone were removed, with the periosteum and articular capsule, the new joint formed was simply a solid fibrous band uniting the divided extremities of the bone; but when the periosteum and articular capsule were left, the articulating extremities of the bones were regenerated in their natural form, and a true articulation, with a more or less complete synovial cavity, formed between them.

The practical importance of these results, for patients suffering from injuries or diseases of the bones, requires no discussion. It has been fully appreciated by surgical practitioners at the present day.

IX. In 1861, Dr. S. Weir Mitchell, of Philadelphia, published the results of two years' researches on the *Venom of the Rattlesnake, and the Treatment of Rattlesnake-bites.** In these researches Dr. Mitchell investigated the anatomy of the fangs, poison-glands, and accessory parts, in the rattlesnake; the mode of production and discharge of the poison, its composition and properties, its action on various species and classes of animals, and the value of different remedies and antidotes. For this purpose he kept several of the living snakes constantly under his observation, and by repeated personal inspection and manipulation familiarized himself with their habits and physiological peculiarities from every point of view. He performed seventy-three experiments with these animals, and observed the action of their poison on frogs, serpents, reed-birds, pigeons, dogs, and rabbits.

The paper, in which the account of these experiments is given, has no superior in medical literature for the clearness and elegance of its style, the abundance of its material, and the precision of its results. It illustrates very distinctly the value of strictly elementary researches as a necessary preliminary to those of a more practical nature.

"When I first," as the author remarks,† "engaged in the study of the venom of the rattlesnake, it was with the intention of ascertaining what value Bibron's antidote possessed. To effect this single end I procured four or five snakes, and proceeded to subject animals to their fangs, and afterward to give the supposed antidote.

"After destroying many animals and attaining only negative

* Smithsonian Contributions to Knowledge. Researches on the Venom of the Rattlesnake, by S. Weir Mitchell, M.D., Washington, 1861; and an article on the Treatment of Rattlesnake-bites, in the North American Medico-Chirurgical Review, Vol. V., p. 270.

† N. A. Med.-Chirur. Review, Vol. V., p. 270.

results, I began to perceive that I was working in the dark, and that it was altogether impossible to obtain useful results, without possessing definite knowledge as to the nature of the venom, the mode of its formation and ejection, and the whole natural history of the disease to which it gives rise."

It would be difficult to find a medical treatise which should illustrate more fully than this the judicious caution and reserve which guide the physiological experimenter, and which enable him to avoid the sources of error that lie in his way. The constant employment of comparative and counter-experiments, and the frequent variation of the methods employed, indicate the care and faithfulness of the investigations, and inspire a well grounded confidence in their results.

In the study of antidotes and remedies, Dr. Mitchell shows the existence and origin of two classes of fallacies, which have heretofore vitiated much of our knowledge on this subject.

First, *Fallacies arising from want of exact knowledge as to the secretion of venom, and the mode in which the serpent uses its fangs and ejects the poison.*

Second, *Fallacies arising from want of information in regard to the natural history of the disease caused by the venom of serpents.*

Finally, the paper shows the comparative value of the various antidotes in vogue; the usefulness of the "intermittent ligature," in checking and controlling the entrance of the venom into the circulation; and the true method of employing stimulus to sustain the patient and preserve him from the fatal effects of the poison.

X. I shall refer in detail to only one more instance of the usefulness of these experiments, viz., the origin and prevention of *Parasitic Diseases.* There are certain parasites which infest the human body, some of which are exceedingly troublesome, and others often fatal. One of these is the tapeworm. The tapeworm, as we know, is derived from eating pork which is infested with another parasite, viz. cysticercus. But the cysticercus is so different from the tapeworm in size, structure, and appearance, that no connexion was formerly supposed to exist between them. The identity of the two was discovered in Germany about the year 1845–50, by Siebold and Küchenmeister.* By experimenting upon mice, rabbits, dogs, and cats, these naturalists found that certain species of cysticercus and tænia were identical with each other, and that a cysticercus in the flesh of one animal, when eaten by another, became developed into a tapeworm in the intestine of the second. This discovery led, soon afterward, to the important result that the human tapeworm is derived from the cysticercus of the pig; and showed the way in which infection by this parasite may be avoided in the human species.

* Transactions of the Silesian Association for National Instruction, Scientific Department. Session of July 7th, 1852.

A more recent and striking instance of the same kind is that of the trichina spiralis. This worm has been known, since 1832, as infesting, sometimes in large numbers, the voluntary muscles of the human subject. It was found on many occasions in dissecting-room subjects and in hospital patients, and was examined by Owen, Henle, Bischoff, Luschka, Harrison, and Farre. But in these cases there was no evidence of the patient having suffered anything from the presence of the worms; and, notwithstanding their great numbers, it was the universal opinion of the profession that the trichina spiralis was a harmless parasite in the human muscle. Nothing was known of its origin or mode of development.

But a little over ten years ago, certain physiologists in Europe began to experiment with these parasites, and, by administering flesh infected with trichina to pigs, dogs, and rabbits, they found that the worm became further developed in the intestine of these animals, and that it there began to acquire a perfect sexual organization. In 1859, Professor Leuckart, in Giessen,* continued the experiments, and showed this development to be complete; the muscles of the second animal becoming infested with the progeny of the trichina, which he had swallowed with his food.

This was the state of our knowledge when, in the year 1860, the members of a family in Dresden were taken ill with symptoms of intestinal irritation, fever, and general pains. One of them, a servant girl, died; and, at the autopsy, her muscles were found to be full of trichina spiralis. These infected muscles were examined by Dr. Zenker and Professor Virchoff, who administered a portion of them to a healthy rabbit. This animal died at the end of a month, and was found to be also infected with trichina. A part of his flesh was given to a second rabbit, who also died, infected, four or five weeks later. The flesh of the second rabbit was then given to a third, who became infected in the same way, and also died at the end of a month. In the meantime, inquiries had been made at Dresden, and a part of the pork, which had been served as food to the family shortly before they were taken ill, was examined, and found to be infested with trichina.

By these investigations, the medical profession became aware of the existence of a fatal and heretofore unknown disease, and at the same time were made acquainted with its source and the manner of its production. Undoubtedly, many cases of trichinosis had happened before 1860; but they were not understood, and the patient was supposed to be laboring under some form of fever or rheumatism. We now know that the trichina, when first introduced into the system, creates a violent disturbance by irritation of the intestine, and by penetrating the tissue of the voluntary muscles. It is often fatal in this stage; but if the patient survive the first five or six weeks, the parasites become encysted in the

* Untersuchungen über Trichina spiralis. Leipzig, 1860.

muscular flesh, and remain there, quiescent and comparatively harmless, for an indefinite period. It was these old and encysted specimens which were first observed, and which could not be referred to any previous illness.

Other experiments have since shown how trichinosis may be detected in pork, and what precautions are necessary to prevent infection from pork used as food.

The importance of this subject may be further estimated from two facts: First, that since 1860 several epidemics of trichinosis have happened in Germany, in two of which over five hundred persons were affected, of whom one hundred and twenty died;* and secondly, that pork in this country is also subject to trichinosis, as appears by the report of a committee of the Academy of Sciences, of Chicago, published in the Chicago Daily Republican of April 13, 1866. This committee examined the flesh of 1,394 hogs brought to Chicago, and found twenty-eight of the number, or one in fifty, infected with trichina. Cases of fatal poisoning from this cause in the human subject have also happened in this country, two of which have fallen under my own observation.

It is to experiment upon living animals, therefore, that we owe our knowledge of this new and dangerous disease, always liable to present itself to the medical practitioner.

In conclusion, there is one point to which I would allude, which seems to me of decisive importance. It is sometimes asserted by those who oppose physiological experiment, that there are other sources of information more legitimate and allowable, and that the chief of these is the observation of the phenomena of disease met with in the treatment of the sick. But do those who take this ground appreciate what they really inculcate when they recommend that we should rely upon this source of knowledge, and neglect experimentation? Do they overlook the fact that such a course is simply *waiting for accidental experiments* to help us in our inquiries? What was the case of Alexis St. Martin but an experiment, accidentally arranged, by a gunshot wound? When we are called to a case of disease, we do not wish to manipulate and experiment with it; we wish to cure it. In order to cure it, we must know its pathology, and understand it beforehand. And if we do not so understand it, then our observations and manipulations in that case are experiments on the living man, and cannot be regarded as anything else. Take the case of the Facial nerve and the Fifth pair. Sir Charles Bell himself says† that, previously to his time, as the result of operations on the face for tic douloureux, "after fifty years of experience, we remained ignorant of the distinction in these nerves."

* Epidemics of Hedersleben and Quedlimburg. L. A. Gosse; Des Trichines spirales, Genève, 1866, p. 30.

† Nervous System of the Human Body, p. 77.

It is true that, in many instances, our observations in disease, and our attempts for its cure, must necessarily be of the nature of experiments upon man. But when experiments upon animals have led the way, and have cleared up, in advance, some of the important points in physiology or pathology, they can only be regarded as fortunate and precious aids in our study of disease. The assistance which this method of investigation may be to us in the future can be judged of by what it has done in the past; for the science of Medicine is one which is constantly advancing, as each generation receives the benefits and feels the impetus communicated by its predecessor.

I have enumerated above the more striking improvements in our knowledge due to physiological experiment, and those most directly productive of practical results. Every medical man, however, will comprehend that these form but a small proportion of the acquisitions which we really owe to it. An immense number of details in every department of physiology have also been learned from this source, which were not directly connected with any especial practical point in medicine or surgery. But they have none the less served to enlarge and complete our physiological knowledge, and to bring it, in numberless minor particulars, into harmony with the truth. And there can be no doubt that the greatest value of physiology, as connected with medical and surgical practice, is the influence which it exerts in a general and intangible way;—by making the practitioner acquainted with the natural functions of the animal organs, and their variations within physiological limits, so that he is enabled to study, with a more intelligent motive and a clearer conception, the processes and phenomena of disease. It is this reasonable and more intelligent method of study which distinguishes the science of Medicine, as a whole, at the present day, and which will undoubtedly be productive of equal improvement in the future.

The benefits of a physiological discovery, accordingly, are not confined to the particular point with which it is especially connected; but it reacts on other similar questions, and raises new points of inquiry, or reconciles previous discrepancies; and its greatest advantage is, after all, that it enlarges, in a general way, the boundaries of physiological knowledge. If I have confined myself, therefore, in the foregoing pages, mainly to instances in which experiment has led directly to practical results, it is because the value of such instances may be more easily and rapidly comprehended, and may be presented with greater preciseness and detail; and because they show, in a clear light, one aspect of the importance of physiological studies, and the means by which they are successfully pursued. If it had not been for experiments on living animals we should, at this day, be ignorant of the circulation of the blood; ignorant of the nature and mechanism of respiration; of the properties and functions of the nervous

system; of the operation of transfusion; of artificial respiration; of the origin and pathology of parasitic diseases; of the best treatment for serpent bites and other venomous wounds; of the office of the periosteum in the regeneration of bone;—perhaps of the Hunterian operation for aneurism; and, for all that appears to the contrary, we should still be cutting the seventh pair of nerves for the cure of tic douloureux.

I have thus endeavored to indicate some of the principal benefits which have resulted to scientific and practical medicine from this mode of investigation; and to place it, both in regard to its objects, its methods, and its results, in the position to which it is entitled as an invaluable assistant to the medical art.

STATED MEETING, JANUARY 2, 1867. DR. JAMES ANDERSON, PRESIDENT, IN THE CHAIR.

IN consequence of some informality in announcement, the annual election was postponed to the 10th instant.

Drs. Everitt Herrick, James H. Pooley, James H. Anderson, and James L. Brown were elected Resident Fellows.

The President announced the decease of Dr. Horace Green; after which, DR. PEASLEE said:

MR. PRESIDENT:—As a friend, and for ten years also a colleague of Dr. Green, I feel that I should not allow the present occasion to pass without some remarks *in memoriam* of our departed associate.

Dr. Horace Green was no ordinary man. His death covers a large circle of social and professional friends with gloom, and by it this Academy is deprived of one of its most distinguished members. At home and abroad his valuable labors in his special department have been recognized and appreciated, and henceforth his name will be enshrined in American medical history, and he be reverenced as a large contributor to the advancement of the profession to which his energies were devoted.

I deem it proper on such an occasion as this, to advert to some of Dr. Green's characteristics, both as a physician and as a man.

As a physician, he was a bold and self-reliant practitioner. His self-reliance was doubtless partly due to his early education; but he was also compelled to rely mainly on himself, from the fact that he was, I may say, the *pioneer* in the application of local remedies to the respiratory passages. His work on Bronchitis, published twenty years ago, was the first systematic work on that subject. The small and fragmentary work of Trousseau and Bellocque had preceded it, I think, by about five years. The opposition also with which he at first met, required no small degree of boldness and self-reliance to overcome and outlive it.

In his intercourse with his professional brethren, Dr. Green was

ever frank and courteous, and incapable of any dishonorable act. He was always ready to show to any physician who called upon him, his entire method of practice, and particularly to answer any objections which had been raised against it; and many a fair-minded physician who had entered his office with a prejudice against him and his methods, has left with a thorough conviction of his correctness. He was, nevertheless, for a time accused in certain quarters of quackery—a charge certainly not to be expected against a man who had published the following candid sentences in the preface to his work, before mentioned:

"In the following pages I have presented a series of observations and facts with regard to the phenomena of disease, and the effects of remedies upon that disease, which it may be in the power of every practical man to verify or disprove.

"I ask then that the inquiry be fairly made; and if it shall be found that the conclusions which have been adopted * * * * are unphilosophical and untenable, let them be discarded."

Some practitioners tested Dr. Green's conclusions, and found them valid; but some also denounced them, without examining them.

No motives of mere policy ever actuated Dr. Green in his professional relations. He acted always on a thorough conviction of the truth of what he professed, respecting his peculiar method of treatment. Had he been a politic man he would not have gone to Bellevue Hospital, as he did, to attempt to pass the sponge-probang into the tracheæ of patients who had never before been operated on, without previously doing himself the justice to explain that it is generally necessary to accustom the laryngeal mucous membrane to the contact of the instrument before it would pass through the rima glottidis. For he was fully aware that most of those present did not admit that this operation is ever possible, except by mere accident. But he only thought of the practical importance of the operation he professed frequently to have accomplished, and trusted entirely to his experience against the odds arrayed against him. That experience, however, did not, even under these circumstances, fail him.

Dr. Green had nothing aggressive in his nature. He never pursued even an adversary; but waiting rather till some conjuncture threw him in his way, he then both rebuked the latter and vindicated himself in a way which his opponent would not forget.

I shall never forget one scene in the life of our departed fellow. I allude to the evening when Dr. Green stood in this hall, arraigned, as it were, on a charge of having killed a patient; conscious as he was, both of his entire innocence of the accusation, and of the fact, that not a few were ready to believe the charge to be true. But the memory of each one here to-night retains enough of the particulars of the Whitney case. I only add, that the verdict of the profession on that case has long since been pronounced; and it is not such that the friends of Dr. Green

would wish to have it reversed. But, Mr. President, could you have seen the suffering in that family as I did; could you have known the anguish of an affectionate and devoted wife, during the days and weeks while that question was agitated, you would have asked yourself of what avail are our struggles for a professional reputation, if a mere breath can thus jeopardize it; and, cruelest of all, a charge from such an unexpected quarter?

Dr. Green had no admixture of vindictiveness in his character. He bore these and other misrepresentations with dignity; and in all my intimacy with him I never heard him apply a spiteful epithet or expression to a single one of his traducers. He never doubted but that a short lapse of time would make all right for him, and used to say he could afford to wait.

But the charge I have just alluded to grieved him to a degree, and produced a shock on his nervous system, from which, I think, he never completely recovered. And from that time I trace the beginnings of the malady which destroyed him. How fearful a responsibility to himself does he assume, who deals lightly with another's professional reputation!

I have alluded to Dr. Green's indisposition to pursue even an adversary. On the other hand, he could not make an effort to conciliate one, when he was himself conscious of being in the right. He had such a supreme contempt for a want of integrity, that when he had once detected it in another, he naturally eschewed him in every way, except so far as common courtesy compelled to the other direction.

As a man, Dr. Green was also kind-hearted, generous, and transparently honest; though by such as could not understand him, he was represented to be a crafty, deceptive, and calculating man. As a citizen, Dr. Green was liberal, high-toned, and loyal; and in his family, both as a husband and a father, he was affectionate, indulgent, and considerate, and was beloved as but few men are.

Finally, and best and greatest of all, Dr. Green lived and died a sincere, consistent, and humble Christian. And who of us shall leave a higher or a purer record than he, when we in our turn shall follow him?

The following resolutions were then proposed by Dr. Peaslee, and adopted by the Academy:

"*Resolved*, That the New York Academy of Medicine proudly claims the late Dr. Horace Green as one of its most distinguished fellows, recognizing him as one of its active and most useful members, as an accomplished physician, a bold and independent writer, a successful teacher, a courteous gentleman, and an honest man.

"*Resolved*, That with unfeigned grief we regret his loss, as one of our associates, as a member of our noble profession, and as a fellow man; and we hereby tender to the bereaved family and friends our sincere sympathy and condolence.

"*Resolved*, That these proceedings be entered on the minutes

of the Academy, and that a copy be transmitted to the family of the deceased."

Dr. Griscom offered a resolution extending a cordial welcome of the Academy to its associate member, Dr. J. Marion Sims, on his recent return from abroad, which was unanimously adopted.

The Academy then adjourned.

Adjourned Meeting, January 10, 1867. Dr. James Anderson, President, in the Chair.

ANNUAL ELECTION.

The election resulted in the choice of the following officers:

President, Dr. Alfred C. Post; Vice-President, Dr. Isaac E. Taylor; Trustee, Dr. Wm. B. Bibbins; Committee on Admissions, Drs. G. M. Smith and Ellsworth Eliot; Committee on Medical Education, Drs. E. R. Peaslee and F. H. Hamilton; Committee on Medical Ethics, Dr. G. Buck.

The Academy then adjourned.

Stated Meeting, January 16, 1867. Dr. James Anderson, President, in the Chair.

Dr. Quimby, of Jersey City, by invitation, presented a child two years of age, his own daughter, born with aggravated double talipes varus, and treated with entire success during the early weeks of infancy, by extension with adhesive plaster.

Dr. Post followed in detail of the partial application of the same method to a case recently treated by him. After dividing the tendo-achillis on each side, and applying the usual bandages, erysipelas occurred in the part, and the child being in a delicate state of health, the integument sloughed. During the period of sloughing Dr. Quimby's plan was adopted, modified by the employment of one strap instead of three, with very good results.

Drs. Everitt Herrick, James H. Anderson, James H. Pooley, and James L. Brown, were inaugurated Resident Fellows; and Dr. C. E. Brown-Séquard was elected a Corresponding Fellow of the Academy.

After appropriate addresses by the retiring President and the President elect, the Academy adopted the following resolution, offered by Dr. Underhill:

Resolved, That the thanks of the New York Academy of Medicine be tendered to Dr. Anderson, for his faithful, energetic, and eminently successful administration as presiding officer for the last six years, as well as for his urbane and courteous manner to all its members.

The Academy then adjourned.

STATED MEETING, MARCH 6, 1867. DR. A. C. POST, PRESIDENT, IN THE CHAIR.

DR. J. C. DRAPER exhibited a photograph of a specimen of ancient matting, accompanied by the following explanation:—

Photograph of a Specimen of Ancient Matting, from Petit Anse Island, near Vermillion Bay, coast of Louisiana.
Presented to the Smithsonian Institution by J. F. Clew, Esq., May, 1866.

Petit Anse Island is the locality of the remarkable mine of rock salt discovered during the Rebellion, and from which for a considerable period the Southern States derived a great part of their supply of this article. The salt is almost chemically pure, and apparently inexhaustible in quantity, occurring in every part of the island (it is about five thousand acres in extent) at a depth below the surface of the soil ten to twenty feet.

The fragment of matting here photographed, was found near the surface of the salt, and about two feet above that were the remains of tusks and bones of a *fossil elephant*. The peculiar interest in regard to the specimen is in its occurrence *in situ* two feet below the elephant remains, and about fourteen feet below the surface of the soil, thus showing the existence of man on the island prior to the deposit of the soil over the fossil elephant. The material consists of the true brake of the common Southern cane (*Arundinaria macrospermum*) and has been preserved for so long a period both by its silicious character and the saline condition of the soil.

The Academy then adjourned.

STATED MEETING, MARCH 20, 1867. DR. A. C. POST, PRESIDENT, IN THE CHAIR.

THE RELATIONS OF UREA TO MUSCULAR ACTION, ETC.

DR. J. C. DRAPER said:—

MR. PRESIDENT:—We are all aware that to within a recent period urea has been regarded by the majority of physiologists as the product of muscular disintegration. It was also satisfactorily demonstrated as the result of disintegration of the nitrogenized food.

In 1856, the year in which I graduated, I made this the subject of my thesis published in the New York *Journal of Medicine*, February, 1856, showed that urea merely represents the excess of nitrogenized material consumed, and is not the indicator of the amount of muscular action.

In a second paper read before the Academy of Medicine, May 24, 1864, it was shown that the effete materials arising during muscular action are evacuated by insensible perspiration by the skin and lungs, and that the loss by these channels is much greater than by the kidneys.

Drs. Fick and Wislicenus, now also assert that muscular force is the result of oxidation of fats. They regard a bundle of muscular fibres as a species of machine formed of nitrogenized material, as an engine is made of iron and brass, and as coal is the origin of force in the latter, so the combustion of fat is the producer of force in the former; and as in the engine there is a slow gradual rust and wear taking place, so in the muscle fibre there is also a similar destruction of material which bears an infinitely small ratio to the force produced.

DR. HUBBARD'S TRUSS.

Dr. Hubbard, of Illinois (by permission), exhibited a truss, of which he claimed to be the inventor; claimed also that it possesses superior advantages over any other truss he had seen;—being very simple in its construction, the spring made of simple steel annealed in oil, giving the instrument only a certain amount of spring, about one and a half inches, susceptible of being moulded into two or three sizes, and bent or straightened to secure a given amount of pressure. He thought that many of the ordinary steel trusses were injurious from the great amount of pressure exerted upon the hernia; thought his superior in this respect. The Doctor then introduced a patient, and demonstrated the manner of applying the truss.

The President stated that in some respects this truss was like that of Eagles, of Birmingham, England.

Dr. Hamilton said that the instrument had been in use a long time; that the annealed spring had been used in a multitude of trusses, and could be found at Tiemann's; and to his mind there was not a new point in the instrument here exhibited. But this, he said, was not his object in rising; without intending any disrespect to Dr. Hubbard, he was opposed to patent medicines, and likewise to patent instruments.

Dr. Foster had seen and examined, during the last thirty years, a great many trusses, but this he thought, in some respects, superior to any he had before seen.

The Academy then adjourned.

Stated Meeting, April 3, 1867. Dr. A. C. Post, President, in the Chair.

Dr. Peaslee presented an ovum expelled with the membranes entire, at five and a half months. The Doctor said, "I have never myself seen one older than that, though our Secretary, Dr. Chamberlain, had a case where it occurred at seven months, and

the child lived twenty-one days. Physiologists have also mentioned instances in which the ovum has been expelled entire, at the full term. The woman was in this instance a primipara, and had had very slight hæmorrhage for four days before the ovum was expelled. No opportunity had been afforded for examination, but I think it to have been a case of placenta prævia, from the appearance of the placenta.

"The membranes fitted with great closeness around the fœtus, so that there was not more than an ounce of liquor amnii within them. The extremities of the fœtus were thus pressed out of their normal position.

"I regard this as a case of interest, inasmuch as it shows that Dr. Simpson's statement, '*that the fœtus has no definite position in the uterus previous to the sixth month*,' is liable to exceptions, since in this case it was impossible that the fœtus could undergo any change, the membranes so closely fitting.

"It is doubtless the general fact that the uterus is nearly of a spherical form, from four to six months, and contains a large amount of liquor amnii in proportion to the size of the fœtus, in which the latter floats, and may be displaced in any direction. By or before the sixth month it begins to assume its ovoid form, and from that time onward the head is in the lower extremity of the cavity."

Dr. Post inquired if the greater weight of the cephalic extremity of the fœtus was not the cause of the normal position of the fœtus after six months?

Dr. Peaslee replied, that that had been assigned as a reason, but he believed the true cause to be, the fact that the larger extremity of the ovoid is the upper end of the uterine cavity, and that the motions of the fœtus being performed by its extremities, would naturally place them in the part where they may have the greatest motion; if, however, as in the present instance, the membranes fit the fœtus very tightly, this agency cannot be exerted. In all such cases as the present, the reason for the expulsion entire is of course the fact that the membranes were not ruptured; and the reason for their non-rupture is the exceedingly small amount of amniotic fluid. In Dr. Chamberlain's case the membranes fitted as tightly as in the present.

Was the small amount of liquor amnii also the cause of the premature delivery? Dr. Peaslee thought it very probable, inasmuch as the motions of the fœtus would be, as it were, directly impressed upon the interior of the uterus, and would not be unlikely to excite premature contractions.

There was very slight hæmorrhage in this case, although probably a case of placenta prævia; since during the contractions of the uterus it would be compressed between the interior of the uterus on the one hand, and the fœtus on the other.

The Academy then adjourned.

STATED MEETING, APRIL 17, 1867. DR. A. C. POST, PRESIDENT, IN THE CHAIR.

CLEFT PALATE.

DR. KINGSLEY, of the College of Dentistry, by invitation, made the following remarks:

MR. PRESIDENT—As is known to most of the gentlemen present, I have devoted some years to the treatment of congenital cleft palate with artificial appliances, with the view of superseding the surgical operation of staphyloraphy. I have had the honor to appear before the Academy on several occasions in connection with this matter, and have stated heretofore substantially, all my views upon that subject. On those occasions I presented to the Academy patients who had been wearing an appliance for a number of weeks, or months, and who had shown very great improvement in their articulation as the result, under the use of the appliance, but that improvement could only be known fully to those who were cognizant of the defective articulation before any attempt was made to remedy it. My own experience shows that there is a very great difference in the articulation of patients for whom nothing has been done. In some cases where the fissure is exceedingly slight the articulation is very defective; and in many other instances where the fissure is very large, involving the entire soft palate and a portion of the hard palate, the articulation is remarkably distinct, considering the defect; so that we may not always infer that the size of the fissure is so much the cause of the defective articulation as in some other cases which may not now be understood.

Dr. K. here introduced a patient with a very large fissure, the largest the Doctor had ever seen without hare-lip, whose articulation was very defective. He also exhibited an appliance, just completed for him to wear, and in order to illustrate the success of this treatment he proposed to present the same patient again in the fall, after having worn the appliance, with proper training.

Drs. John F. Townsend, Stephen Rogers, E. S. Dunster, and W. T. White, were inaugurated Resident Fellows.

The Academy then adjourned.

STATED MEETING, MAY 1, 1867. DR. A. C. POST, PRESIDENT, IN THE CHAIR.

SUSPENDED ANIMATION.

DR. WORSTER related the particulars of an accident occurring in his own practice, during an operation for hæmorrhoids. The patient having been placed under the influence of chloroform, the doctor proceeded with the operation, and when about to remove the third tumor he discovered symptoms of suspended animation.

The assistant, an apothecary and brother of the patient, had been intently watching the operation, while the patient's face was buried in a napkin saturated with chloroform until there was only an occasional slight pulsation of the heart. Upon turning the patient on his back and sliding him obliquely from the bed at an angle of about forty-five degrees, the head downward, he soon rallied without any unpleasant effects. Dr. W. believes that this treatment was first suggested by Professor Nelaton, of Paris, and that the rapid recovery is in consequence of a rush of blood to the head.

TRAUMATIC RUPTURE OF THE UTERUS.

Dr. Dunster related the case of a woman who at the full term of pregnancy was gored by a cow, the abdominal and uterine walls torn open, and the child thrown uninjured to the ground. This occurred at West Point. About an hour after the accident the patient was seen by Dr. Marsh, of the U. S. Army, who found the uterus everted, and the placenta still attached. He separated the placenta and placed the uterus in its normal position, but the patient died about two hours after receiving the injury.

SLOUGHING OF THE EYE.

Dr. Garrish related the case of a little girl that had been treated antiphlogistically for a violent inflammation of the right eye, which terminated in sloughing. Dr. G. being requested to see the patient, found that the cornea had sloughed, and upon examination, discovered a small piece of letter-paper beneath the upper lid. He added that this was the third case of sloughing of the eye, from a similar cause, that had come under his observation, in which the attending physician had neglected to evert the lids for the purpose of ascertaining whether the inflammation depended on the presence of a foreign body.

Dr. Post reported a case of dislocation of the head of the humerus into the axilla, which was reduced by joint manipulation and extension.

BIFID UTERUS.

Dr. Peaslee said:—A lady called on me several weeks since, from a distant city, with an abdominal tumor and a letter from her physician to the following effect:—

She was thirty-eight years old, had had five children, and had been treated for uterine disease for more than a year past. During all this time the os-uteri was largely patulous, and the canal of the cervix also much dilated; and as she constantly had a leucorrhœal discharge, applications had often been made to the uterine cavity. This treatment had been continued, at intervals of a few days, up to the time when she left home.

The tumor in question was first discovered last November, and menstruation had not appeared since about October 1. As the tumor increased, the uterine cavity became more elongated, as tested frequently by the sound, and was now, at least, eight or nine inches long. Some weeks since she thought she felt the motions of a fœtus in the tumor, but was not encouraged in this idea. The apparent movements, however, still persist, and are now even more apparent to her.

On examining the patient, I found an elongated ovoid tumor, extending from within the pelvis, apparently to the level of the umbilicus; but it presented an irregular and lobulated feel superiorly, and in front; was somewhat tender on pressure, and lay still further to the right even than the pregnant uterus is accustomed to do. It did not change its position when the patient turned on the left side.

On pressing the hand steadily upon it the motions of a fœtus were now unmistakably felt.

This was then a case either of extra-uterine pregnancy; or, what is still less common, of bifid uterus, with a fœtus in the right half of the organ.

The head of the fœtus could be easily made out through the abdominal walls, lying in the right iliac fossa, and being apparently very thinly covered. Per vaginam also, the head could be easily felt, and seemed to be separated from the finger by the thickness of the upper part of the vaginal walls alone. But she was probably six months advanced in pregnancy, and had had some of the signs attributable to extra-uterine pregnancy.

Passing the index-finger into the patulous os-uteri and through it, I found, as it entered the uterine cavity, that it again felt the fœtal head, separated from it by a thickness merely like that of common paper; and carrying the point of the finger half an inch higher, it came into contact with a free, thin edge, like that of one thickness of paper lying across another.

I decided that the first thin layer just mentioned consisted of the membranes around the fœtus; and that the second was the border of the septum, between the two halves of a bifid uterus.

I did not see the patient again for a week or ten days, when her husband called on me, stating that his wife was flowing. I found that the liquor amnii only was escaping, though there had not been any uterine contraction; and now the fœtus could be directly felt by the finger passed to the right, into the uterine cavity.

In twenty-four hours the uterine contractions came on, and in three hours more she was delivered of a six months' fœtus, which, of course, did not live. She has made a good recovery.

It is not often that we have the opportunity of diagnosticating pregnancy in a bifid uterus, from extra-uterine pregnancy.

I am happy to be able to say, however, that the profession will be at fault if not generally more familiar with this class of anomalies, after the publication of Dr. Kammerer's translation of

Klob's Pathological Anatomy of the Uterus; which is now nearly ready for the press.

The Academy then adjourned.

STATED MEETING, MAY 15, 1867. DR. A. C. POST, PRESIDENT, IN THE CHAIR.

A NEW INHALER.

DR. D. H. GOODWILLIE presented an instrument for the inhalation of ether, or chloroform, which he describes as follows:—

a, Bottle filled loosely with a sponge.

b, A tube passing through the cork to the centre of the sponge.

c, A tube to convey the vapor from the bottle to the inhaler.

A, Faucet, composed of hard rubber, containing the inhalation, and exhalation valves and air-passage.

C, Face-piece fitting over the mouth and nose (two sizes). Inhaler made of hard rubber.

FIG. 2.—*Explanation of the faucet containing the valves:*—

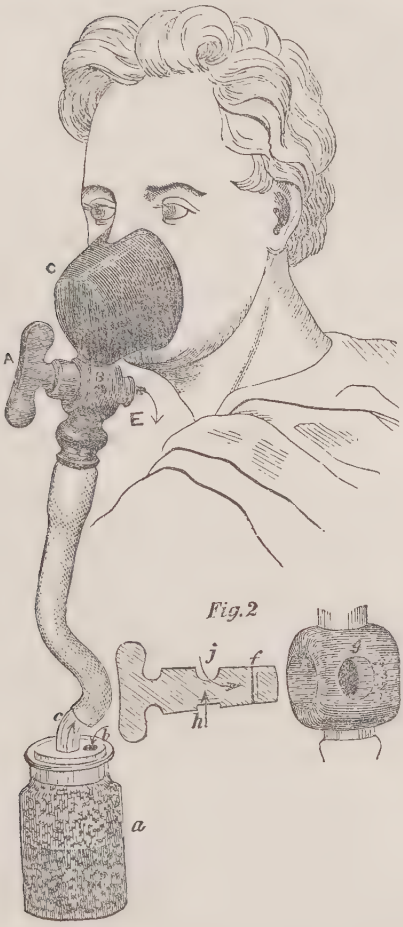
h, Inhalation valve for the passage of vapor.

f, Exhalation valve for the breath to pass out.

j, Passage for the vapor inhaled and the breath exhaled.

g, Fresh-air-passage, divided into (5) five parts, with an index to show the quantities of air or vapor inhaled at any given time.

The inhalation valve *h*, and the air-passage *g*, are of the same caliber, and as the faucet revolves in the administration $\frac{1}{6}$ of a circle, the vapor increases, while the air decreases to that point where we have the maximum amount of vapor to the minimum amount of air considered safe to inhale. This point is indicated at 4 on the index (4 parts of vapor to 1 of air). The relative proportions of vapor and air are changed by the revolution of the faucet from 1 to 4, or 4 to 1. The first (1 to 4) increases the vapor, and in the same proportion decreases the air. Revolving from 4 to 1, increases the air, and decreases the vapor. When the passage of the vapor is at maximum there is more than one part of air inhaled, as to this may be added a small proportion of air inhaled from the bottle with the vapor. Let the patient now inhale with the inhalation valve open, the air passes into the bottle (at *b*), causing the liquid to vaporize and pass out (at *c*), into the inhaler to be breathed, the amount of vapor or air being entirely regulated by the revolution of the faucet on the inhalation valve (*h*, Fig. 2), or air passage (*g*, Fig. 2), which amount is known by the index. For example, revolve the faucet to 2 on the index and 2 proportions of vapor to 3 of air are inhaled. The bottle should never be more than half full, so as to allow the air to vaporize the liquid on the upper half of the sponge. It is intended to have the liquid remain in the bottle and to be replenished when necessary. This will be found to be a simple, safe, economical, and practical method of administering all anæsthetics; air (in chloroform or ether) can at



all times be inhaled with a sufficient quantity of vapor to produce anæsthesia. As the vapor can be inhaled gradually, there is less spasm of the epiglottis, and struggling, and the patient comes calmly into a state of narcotism.

A very small quantity of ether or chloroform is used to produce anæsthesia; and to keep it up, any desired quantity can be given.

Dr. Post wished to inquire as to the time it took to produce anæsthesia with this instrument?

Dr. Goodwillie replied, that the time to produce anæsthesia was under the control of the administrator, as any safe quantity of the vapor could be readily given; but thought it much better

to commence the administration gradually at first, and found it produced less coughing, struggling, and sickness. In his last case, a strong, healthy man, he produced anæsthesia in seven minutes.

Dr. Howard asked if it measured the total amount of the anæsthetic used?

It was answered that it could be ascertained by a graduated scale on the bottle.

In answer to a question by Dr. Chamberlain as to the quantity of chloroform required to produce anæsthesia, Dr. Goodwillie said he had produced anæsthesia with twenty minims; and in reply to a question by the President, that he had not accurately measured the amount of ether, but as there was no loss it took a very small quantity.

Dr. John C. Draper then read the following paper on

COFFEE AND ITS ADULTERATIONS.

Of the beverages that have been introduced into the dietary lists of civilized nations since the seventeenth century, there is not one of greater importance than that which both the rich and poor find so gratifying to the palate, and so necessary for the perfection of the breakfast, or first meal. Deprived of the morning cup of coffee, throughout the day we feel a diminution in the accustomed vigor and ease with which work is accomplished, and many will seek relief from the lassitude by which they are oppressed by indulgence in some form of alcoholic stimulant.

Coffee, therefore, is of interest, not only from a physiological, but also from a moral point of view; and it consequently becomes necessary that every feasible and proper precaution should be taken to preserve it from the manifold processes of adulteration to which we shall find it is almost universally subjected. It is no justification of such adulterations that they are said to be innocuous; for even if the assertion was true, the adulterations almost invariably consist of valueless substances, which are thus palmed on the poor at an enormous profit to the seller; and the laborer is not only cheated of the stimulant and nutritive material a cup of good coffee would furnish, but his pocket is also robbed of hard-earned wages by the purchase of a worthless article, which he might himself prepare for one-third or one-fifth the price demanded.

When properly made, coffee is a solution in hot or boiling water, of certain constituents of the roasted seeds of the *coffea Arabica*, a shrub or tree of the order *Cinchonaceæ*, and a native of Southern Abyssinia, though it also grows wild in Liberia. It was first introduced into Constantinople in the middle of the sixteenth century, and coffee-houses appeared in London only in the latter part of the seventeenth century, since which time this valu-

able beverage has come into general use in nearly all parts of the civilized world.

The appearance of the shrub or tree is described as follows, by Mr. Ellis:—When in good health and full-grown it attains a height not exceeding eight or ten feet in some countries, but in others from fifteen to twenty feet. Its bark is gray, and it is covered with a dark, smooth, shining, and evergreen foliage, the leaves being four or five inches long, of an oblong ovate form, and set opposite each other. It is sown in nurseries, transplanted when about six months old, and the growth checked by topping the plant to favor the collection of the fruit; in three months it comes into full bearing, and in favorable circumstances will continue to bear for twenty years. It flourishes best in a dry soil and a warm location; its flowers are pale, white, fragrant, hardly ever absent, and fade rapidly; its fruit, like that of the cherry-tree, but it grows in clusters; the fleshy part is very palatable, and in the interior the seeds are found, each of which forms two of the grains of commerce. On dry and elevated parts the seeds are smaller, and have a better flavor, but seeds of all sizes improve in flavor, or ripen by keeping. The small grains of Arabia will ripen in three years, but the worst coffee of America will in not more than ten or fourteen years become as good, and acquire as high a flavor as the best we have from Turkey. The superiority of the Arabian seed is doubtless due in part to the fact that the planters in the region where it is cultivated, use a system of irrigation, and turn off the water from the roots when there is a large quantity of fruit nearly ripe, and do not pluck the fruit, but to insure its ripeness shake the trees and collect the berries on cloths spread to receive them.

The principal supply of the coffee consumed in the United States is derived from Brazil and St. Domingo; but there are also other varieties to be found in the New York markets, among which we may mention Mocha, Java, Ceylon or East India, and Maracaibo, La Guayra, or American or West Indian. The Mocha or Arabian coffee is distinguished by the smallness of the seed and its yellow color; Java and East Indian by its light color and large size; while the American and West Indian is intermediate in size, and has a greenish-gray or bluish-gray tint. The seeds of good quality may be distinguished by their being hard, and so heavy that they sink quickly in water; they have a faint coffee-like odor, are free from any smell of dampness or mouldiness, and possess a sweetish taste.

Though the seed is so universally employed for the preparation of coffee, it is not generally known that other portions of the plant also possess similar properties. It is stated by Mr. Ward that the natives of Sumatra universally prefer the leaf to the seed, giving as a reason that it contains more of the bitter principle, and is more nutritious. In the lowlands, coffee is not planted for the berry, not being sufficiently productive, but for the leaf, the

people planting it around their houses for their own consumption.

In order to prepare the seeds for use, they are acted on by heat, in such a manner as to develop the aroma without volatilizing it. This is best accomplished by first roasting them in an open vessel to expel the water, and then completing the operation in a closed rotating roaster, in which the coffee is submitted to a risk fire, and kept in continual movement, so that the seeds are all acted on to the same extent. The roasting should be continued until the grains are of a chestnut-brown color, and sufficiently brittle to be cracked without difficulty, but the stage of carbonization should be carefully avoided. During this operation there is a loss in weight of about twenty per cent., but the bulk increases about 50 per cent. After the roasting is completed the seeds should be allowed to cool slowly, to absorb the aroma, which may be retained, without much loss, by keeping the coffee in a well-closed vessel, from which it may from time to time be removed for use, and the solution prepared immediately after it is ground. Owing to the absorbent power of freshly-roasted and ground coffee, it may be employed as a deodorizer in the sick room, and is often used advantageously for that purpose.

Parkes recommends that the coffee served to troops in service should be previously roasted and ground, and preserved in sealed cans. The advice is no doubt good, if contractors could be trusted, but if such a system were adopted it would be next to impossible to furnish an army even with a poor article, for it would inevitably be adulterated to such an extent as to be useless. There is, however, no objection to serving it roasted and hermetically sealed in cans, as every advantage that is to be desired is gained, and but little opportunity for adulteration furnished, and soldiers would be better satisfied to crush it themselves.

The preparation for the purpose of drinking is usually by the percolator or by boiling, according to the taste and fancy of the individual. On this point the following advice by Parkes is, we think, worthy of attention:—He says: "The coffee must not be boiled, or the aroma is in part dissipated, but if made with water of one hundred and eighty or two hundred degrees the coffee only gives up nineteen to twenty per cent., whereas it ought to yield thirty to thirty-five per cent. of soluble matter. The amount, however, depends also on the kind and degree of roasting. In order to get the full benefit of the coffee therefore, after the infusion has been poured off, the grounds should be well boiled in some more water and the hot decoction poured over fresh coffee, so that it may take up aroma; the coffee thus partially exhausted can be used on the next occasion for boiling. To clear the coffee some cold water should be poured in from a little height; the cold water sinks through the coffee and carries down the suspended particles." The solution thus prepared has a specific gravity of

about 1,010, and contains the sugar, dextrin, caffein, oil, and a part of the mineral ingredients of the berry.

The properties of this invaluable beverage are thus summed up by Pereira. It possesses powerful anti-soporific properties, hence its use as a drink by those who desire nocturnal study, and as an antidote to counteract the effects of opium, belladonna, and other narcotics, and to relieve intoxication. In those unaccustomed to its use it is apt to occasion thirst and constipation. On some persons it acts as a slight purgative. It is occasionally useful in relieving headache, especially when of a nervous type. It has also been employed as a febrifuge in intermittents, as a stomachic in some forms of dyspepsia, and as a stimulant to the cerebro-spinal system in some nervous disorders. Dr. Percival and others have used it in spasmodic diseases, as whooping-cough and asthma, and Laennec says, "I have myself seen several cases of the latter disease in which coffee was really useful." It has also been employed with advantage in hysteria. The immoderate use of coffee is said to produce nervous symptoms, such as anxiety, tremor, disordered vision, palpitation, feverishness, and not infrequently, headache.

From the general consideration of the preparation and physiological properties of coffee we now pass to the examination of the methods employed for its adulteration. They may be classified under three heads:—

1st. The mixture of superior with inferior varieties.

2nd. The mixture of good coffee with that which has been damaged by water.

3rd. The addition of exhausted coffee grounds or foreign substances, as chicory, bread, rye, &c.

The first method is, of course, the least injurious, and while the seeds are yet in the green state may generally be detected by an expert without difficulty; when they are roasted it is not so easy; and if roasted and ground, it is next to impossible to discover by any chemical or microscopic examination the extent to which the adulteration has been carried; and it is only when the article is finally prepared for use that its unsatisfactory character is detected.

The second system of adulteration, by seeds that have been damaged, may also be determined by an expert, since the grains are generally saltish, or if they have been washed in fresh water and dried, still possess an unpleasant taste, even after they are roasted. The presence of such material in roasted and ground coffee can only be detected with satisfaction by the inferior character of the solution it furnishes.

From the above passing view of the first two methods of adulteration, we turn to the third, which is by far the most important, since, in all probability, more than nine-tenths of the coffee consumed is sold to the consumer in the roasted and ground state, in which condition it is easy to introduce many foreign substances.

The tests that may be practically employed in the examination of specimens of roasted and ground coffee may be classified under

two heads, 1st, the physical, and 2d, the microscopic, to which we may, if desirable, add a few chemical tests, which are, however, not of any great value, since the physical and microscopic examinations are quickly finished, and furnish all the information that is required.

The aroma and appearance of ground coffee cannot be depended on, for they may be so manipulated as to deceive the closest observer, and in one specimen that passed under my notice during the past winter, and which possessed in perfection the aroma of freshly roasted fine old mocha, there was not a single particle of any kind of coffee, the article in question consisting of a mixture of chicory and bread or biscuit, over which the fumes of roasting mocha had probably been conducted, so as to impart the delicious aroma it possessed, and which could only have been given in some such way.

The substance generally used for the adulteration of ground coffee is chicory, which is at present furnished in the New York market, ready for use, at about fifteen cents per pound, by the quantity, a good specimen of Java in the same state costing about forty-five or fifty cents, and the inferior grades of ground common coffee being worth about 40 cents, so that the retail dealer is able by the judicious addition of chicory to secure a very handsome profit.

When the proportion of this substance is large it may be readily detected by the simple test recommended by Mitchell, which consists in kneading a small quantity of the specimen in the palm of the hand with a little water, when the chicory imparts a gummy feel, and if the quantity is sufficient, the mass may be made to cohere in any desired form; while, under the same conditions, a sample of good coffee fails to cohere, and crumbles to pieces when we attempt to give it form.

In order to employ the physical properties of coffee as a test of its value, I contrived the instrument represented in the adjoining figure, and have found it of great service. It affords at once a number of indications by the observation of which we can not only detect the presence of foreign substances, but also form a very good estimate of the quantity. This instrument consists of a tube (A B), one inch in diameter and eight inches in length, which terminates below in a narrow tube (B C), one quarter of an inch in diameter, and four inches in length; the lower small opening (C) is closed by means of a cork to a certain point, from which it is graduated to tenths of a cubic centimeter; a number of these tubes, with a suitable stand, and a small measure holding one cubic centimeter, complete the apparatus.

When it is to be used, the tubes are closed below, and filled to within half an inch of the upper extremity (A) with cold water (that has been boiled to expel the gas, and kept in a well stopped bottle to prevent its reabsorption). A cubic centimeter of the sample of coffee under examination is then cautiously dropped

on the surface of the water, when we should notice, 1st, whether it sinks quickly or slowly; 2d, what tint and odor it imparts to the water; 3d, the expansion in bulk as measured in the graduated tube; 4th, the change in color of the grounds, and whether they collect loosely or not.

If it is good coffee and a pure article, it floats on the surface for a considerable period, and if freshly roasted and ground will often remain suspended for days, if the tubes are kept free from agitation. In the majority of instances it falls in a few hours, and imparts to the water in the large tube a delicate amber tint and a faint coffee-like odor, each particle as it rests in the small tube retaining its outline, individuality, and color; and the increase in bulk being about one half a cubic centimeter.

If it is chicory it sinks instantly, nearly the whole mass tumbling into the small tube in a single minute, and even in its rapid passage through the water imparting to it a deep brown color, and an odor somewhat like that of liquorice; the tint of the particles is at the same time altered greatly, the expansion of the mass is also very considerable, its bulk in a short time increasing to nearly two and a half cubic centimeters; and the particles losing their individuality seem to fuse together, and form a mass without interstices.

By preparing mixtures of chicory and coffee in different proportions, we find that the first sinks rapidly, while the latter remains suspended for a considerable period; though a small portion may be carried down by the chicory in its sudden subsidence. After a time (which may be shortened, if desired, by agitating the tube), the coffee also sinks, and by noting the proportion of compact and loose sediment in the small tube, and the position of the line of meeting of the dark and light colored solution, we obtain an indication of the proportion of chicory to coffee, which coincides very nearly with that employed in making the mixture. The tint of the fluid in the large tube, and the amount of increase in the bulk of the solid matter, are also of use in arriving at a correct estimate. After the performance of a few experiments with mixtures of coffee and chicory in different proportions, a degree of expertness is obtained which enables the operator to form an accurate estimate of the amount of each ingredient in any sample that may be submitted for his examination.

Not only may coffee be thus to a great extent separated from any chicory with which it is mingled, but nearly all the other substances used for the purposes of adulteration, as exhausted coffee grounds, wheat, &c., also subside rapidly with the chicory and collect in the small tube. If it is desired to determine the character of the matter that precipitates quickly, we must resort to

the microscope, when by the nature of the cells and other structures found, we can soon decide upon the true character of the foreign substances. The presence of chicory is readily and satisfactorily proved, and if peas, beans, &c., are present, they may be easily distinguished by the peculiarities of the starch cells of each. Such microscopic examinations are greatly facilitated by the use of the tubes I have described, for when the subsidence is complete, and we have read off the proportion of adulteration, it only remains to remove the cork gently from the small tube and allow a little of the lower portion of the sediment which contains the foreign matters to escape into a shallow dish, whence particles may be transferred to the microscope slide, and if it is necessary we may in this manner examine layer after layer of the deposit.

In order that some idea may be formed of the extent to which adulterations are introduced into coffees offered for sale in New York, I give a few of the results obtained in the examination of samples taken from different coffee stores during the past winter :—

a 40 cent coffee, 50 per cent. adulteration, consisting of chicory and wheat.

b 30 cent coffee, 30 per cent. adulteration, consisting of chicory and wheat.

c 20 cent coffee, 50 per cent. adulteration, consisting of a little chicory, wheat, and beans.

d 18 cent coffee, 75 per cent. adulteration, consisting of a little chicory, wheat, and beans.

e 30 cent coffee, 50 per cent. adulteration, consisting of chicory.

f 40 " " 25 " " " " "

g 38 " " 33 " " " " "

h 40 " " 50 " " " " "

i 50 " " no adulteration.

j 40 " " all chicory.

k 45 " " 25 per cent., fell rapidly; was probably exhausted grounds.

l 45 cent coffee, no adulteration, was an inferior coffee.

m 50 cent coffee, no adulteration, was a good coffee.

All the above specimens were sold under attractive titles, with the exception of *i* and *m*, which were advertised as Java coffee, and from the character of the solution they furnished, I have no reason to doubt the statement of the sellers, who were respectable grocers. Out of the thirteen samples quoted above, nine contained chicory, four contained wheat, and two contained beans. The percentage of these foreign substances was so large, that the profit to the seller was very great, and in some of the instances the beverage furnished was utterly valueless.

In the specimens in which wheat was found, it is my impression, from the manner in which they acted in the tube, that the adulteration consisted of spoiled biscuit and refuse bread, which could be obtained for a mere trifle. In one specimen not given above, there were masses which presented the appearance of liver altered

by roasting, and which is said by Hassall to be extensively used in London in the adulteration of coffee. Other specimens presented structures with which I am not acquainted, but which were evidently organic in their origin.

In addition to the substances spoken of in this paper, Hassall mentions corn, potatoes, turnips, and sawdust. These I have thus far failed to find in the samples that have passed under my notice, and which were obtained from different parts of our city, the favorites among our dealers being chicory, bread or biscuit crumb, rye, and beans. These are of course obtained at the cheapest rates, and must usually be refuse, which cannot be sold for any other purpose than that of being roasted, crushed, and manipulated into an article to be sold to the poor, who, thinking they cannot afford to buy an expensive coffee, give away their money to the dealer for useless refuse, and the government is defrauded of the revenue it should have collected as importation tax.

In conclusion, it is important to revert to the statement that these systems of adulteration are innocuous, and therefore not worthy of attention, except in so far as they defraud the poor and the revenue. We must not accept such an opinion too hastily, for coffee is not only used as a beverage, but it is also at times a most important and valuable medicinal agent; as, for example, in poisoning by opium, belladonna, and other narcotics. If, in such cases, the antidote ordered by the physician fails to produce its proper effect, on account of the general system of adulteration to which it is subjected, such manipulations become criminal, and can only be corrected by compelling the sellers, under penalty of a fine, to state the true nature of the substance they furnish.

Where rye is used, it is evident that only a poor article is employed, and nothing is more likely than that spurred grain may be thrown into the roaster, when if the roasting is imperfect, a sufficient quantity may escape the action of the heat to produce very disastrous effects on the systems of pregnant women, or may perhaps cause the mortifications which by many are supposed to result from the continued introduction of ergot into the system. Roasted chicory-root is also to be condemned, for it is by no means as harmless as some would have us think, for at first it generally produces nausea; and the statement of Johnston, that when taken frequently in small quantity it causes severe dyspepsia, is one that I have verified by my own experience. From these facts we may fairly conclude that the adulteration of coffee is neither justifiable nor harmless, and the sale of such mixtures under the name of coffee should be prohibited by suitable means. If the public will still insist on purchasing useless and injurious substances for the preparation of a beverage, let them be sold under their proper names, so that when coffee is wanted for ordinary use, or needed for medicinal purposes, the procuration of a good sample may be within the bounds of possibility.

Dr. Garrish stated that he had found the use of coffee in the

treatment of asthma beneficial in his own experience, and recommended it; had known patients relieved, and the paroxysms prevented by its use. He inquired of Dr. Draper if it were not better to grind the coffee in the morning which was to be used the same day; to which the doctor replied, if it was kept in well-closed vessels, retaining its aroma, it would keep for several days; but that by crushing it in a mortar it seemed to retain its aroma, and he thought crushing preferable.

Dr. Van Kleek had used coffee successfully in the treatment of whooping-cough, it having been first brought to his notice in his own family; he had found it to act beneficially in other affections.

Dr. Post had seen good results from the use of coffee in certain forms of headache.

Dr. Downs had frequently used coffee, called pure, which acted upon his kidneys in a very striking manner.

Dr. Post suggested that it might have been dandelion coffee, that herb being known as a diuretic.

ŒDEMA GLOTTIDIS.

Dr. Finnell exhibited a pathological specimen of the tongue and larynx of a man fifty years of age, who left Utica, N. Y., on Thursday last, in the enjoyment of good health. While on board the steamboat to New York city, he slept in an exposed condition, and next morning he complained of soreness of the throat. Thirty-six hours after exposure, suffering from symptoms of suffocation, he went into a saloon to purchase a bottle of wine, and before leaving was requested by the proprietor to take a glass of the wine to try it, after which his difficulty of breathing became much more intense, and he sat down in a chair and soon expired.

The specimen showed œdema in the loose cellular tissue around the base of the tongue and glottis, being most abundant in the aryteno-epiglottidean folds—no œdema in the larynx; it was free—it presented very much the appearance of a part of the application of a blister, the folds of the tissue so overlapping the chink of the glottis that in its recent state it could hardly be seen.

This specimen (the Doctor said) illustrated the great value of scarification, and he did not doubt that had scarification been performed, the man's life might have been saved; the exciting cause of death, however, being the wine.

Dr. Post exhibited an improved tongue-depressor, stating that some years ago, while operating upon the uvula before his class, he remarked that it would be much more advantageous to have three hands; one to hold the tongue-depressor, another to hold the forceps, and a third with which to operate. Next day a member of the class presented a model of a tongue-depressor which could be

held by the little and ring fingers; thus leaving the thumb, index, and middle fingers free to hold the forceps.

Dr. P. had a forceps for the purpose, being a modification of the sponge-holder of Mr. West, of London.

Dr. Garrish said, œdema of the glottis is fortunately a very rare disease; he had seen some eight or ten specimens at the Pathological Society. As to treatment, scarifying was the most useful; scarifying every hour or two, if the symptoms were urgent, and keeping the patient in an atmosphere of steam, from an infusion of marsh-mallow leaves.

The doctor was called to see a young man, seventeen years of age, at Manhattanville, whom he found laboring under the most violent symptoms; the patient was immediately scarified, and the inhalation of steam used, and he went into convulsions; the doctor opened a vein in his arm, and he died in half an hour after the doctor's arrival, in that convulsive condition.

In another case Dr. G. followed a similar course of treatment, but just as soon as he found danger of suffocation, he opened the trachea, and the patient recovered.

Dr. Post held that simple scarification gives perfect relief, that often a single scarification would be sufficient, and thought that the inhalation of steam would *increase* the effusion.

Dr. Downs had a serious case some years since which was scarified, and the patient recovered.

Dr. Bulkley wished to know if the inhalation of steam under those circumstances was not very injurious, as the object in view is to decrease the effusion; he thought the inhalation would augment the difficulty.

Dr. Garrish remarked that all persons with a practical experience were aware of the beneficial effects of steam inhalations in diphtheria, &c.; and he could not see why it was not also beneficial in œdema of the glottis.

Scarifications were known to have been resorted to and the patient die. When scarification failed to give relief, Dr. G. would unhesitatingly perform tracheotomy.

The Academy then adjourned.

Stated Meeting, June 2, 1867. Dr. A. C. Post, President, in the Chair.

Dr. Bell read the following paper on

THE ECONOMY OF HUMAN LIFE.

By A. N. Bell, M.D., Physician to Brooklyn City Hospital.

The chief object of this paper is to show the Economy of Human Life *in New York* at the time of the going into effect of the Metropolitan Health Law. But while this purpose is kept in view, I have not wholly confined myself to it, hence I have abbreviated the title.

The Economy of Human Life has engaged the attention of the

most enlightened communities from the very beginning of learning.

The Romans, during the reigns of Severus, Adrian, Antoninus, and Aurelius, instituted and amended laws for the preservation of human life in conformity with the teachings of Hippocrates and Aristotle. The Roman *census*, instituted by Claudius, in the first century of the Christian era, for war purposes, was kept up for thirteen centuries. But, unfortunately, it comprehended *citizens* only, excluding slaves; consequently the life tables which have been constructed from it, are to be taken as the estimate of the *best* lives only, and not for the whole population. Taken in the aggregate for 1,000 years, beginning at the third century, Berard* estimates the mean duration of the Roman citizen's life at thirty years.

Subsequent to the Roman census, up to the early part of the present century, the observances of the Christian religion alone appear to have kept up a registration of births and burials, and from these the comparative mortality and longevity of different States may be estimated.

The oldest and most complete of these registries thus far examined, are those of Geneva.† Complete registration of births, marriages, and deaths, appears to have been established in that city in the year 1549; but the ages of the deceased were not given until eleven years later, in 1560.

From 1568 to 1579, there is an omission of the ages; but from this latter date, with the exception of 1648 persons who died of the plague in 1615–'16, the registration is complete. At the time of their institution (1549), half of the children born, died before reaching the age of six years, and the probabilities of life were, to every individual born, eight years, seven months, and twenty-six days. During the 17th century, under an increase of population from 13,000 (in 1589) to 17,000, the probabilities of life increased to thirteen years, three months, and sixteen days.

1701–1750, to	27	years,	9	months,	13	days.
1751–1800, "	31	"	3	"	5	"
1801–1813, "	40	"	8	"		"
1814–1833, "	45	"	0	"	29	"

The increase of population and duration of life, had been attended by a progression in happiness; as prosperity advanced, marriages became less numerous and later; the proportions of births were reduced, but greater numbers of the infants born were preserved; and the proportion of adults became greater. In the early periods, the excess of mortality was accompanied by a prolific fecundity. In the ten last years of the seventeenth century, a marriage still produced five children and more; and the probabilities of life were less than twenty years.

Toward the end of the eighteenth century, there were scarcely

* Discours sur les Ameliorations, etc. † Mallet.

three children to a marriage, and the probabilities of life exceeded thirty-two years. In 1834, a marriage only produced two and three-quarter children; the probability of life being forty-five years.

Sweden is particularly distinguished for the accuracy and completeness of her registries.* Births, marriages, and burials, with the number of both sexes, and their ages—in every town and district—were kept with great care more than a century ago; and a census of the living, at every age, was taken once in five years.

In England,† the keeping of parish registers was first ordered by Cromwell, after the abolition of the Pope's authority, in 1538. At first, the registration only included weddings, christenings, and burials. Even this soon fell into disuse. It was again ordered by Edward VI., in 1547, and by Elizabeth, in 1573, and finally by a canon of the church, in 1603. The ages of the deceased were not specified until 1728,‡ and this was the last improvement until 1836! During this long period of time there were many English writers on the importance of mortality statistics,§ but the government could not be aroused to a due appreciation of their utility. The collating, printing, and publishing these documents, were placed under the superintendence and jurisdiction of the ancient corporation of parish clerks.‖ This society was incorporated by letters-patent of the 17th Henry III., in 1239, by the style of the *Fraternity of St. Nicholas.* In 1625, they obtained a decree from the Star Chamber, allowing them to keep a press in their hall for the printing of the weekly and general bills of mortality of the city and liberties of London; and for this purpose a printer was appointed by the Archbishop of Canterbury. A power was thus granted to a society, which, it is hardly necessary to observe, was wholly inadequate to the accomplishment of the medical, political, and moral objects which these bills were calculated to promote. The process of conducting this important service is thus described by Dr. Paris, 1823.

"The church-wardens of each parish within the bills of mortality, appoint two old women to the office of *searchers*, who, on hearing the knell for the dead, repair to the sexton of the parish, to learn the name and residence of the deceased. They demand admittance into the house to examine the body, in order that they may see that there is nothing suspicious about it, and judge of what disease the person died, which they report to the parish clerk.

"Many of the diseases are absolutely unintelligible; such, for instance, as *Headmoldshot; horseshoe head; over-grown head; rising of the lights; liver-grown; twisting of the guts*, &c. In-

* Paris and Fonblanque, from Sussmilch, and others.
† Halley. ‡ Paris and Fonblanque.
§ Graunt, Corbyn, Morris, Dodson, Birch, Price, Burrows, Heberden, and others.
‖ Paris and Fonblanque.

fants dying before baptism are not returned by the parish clerks."*

Another authority, Dr. Heberden, states that the births include only those who are baptized according to the rites of the church of England, by which means all Jews, Quakers, and the very numerous body of dissenters, are omitted. And of the deaths, those in the several hospitals of the metropolis, those buried in cathedrals, abbeys, &c., are all omitted.

The above are fair examples of mortality statistics previous to the beginning of the present century. And although, as in the case of Geneva, Sweden, Holland, Wurtemburg, Paris, and other European states and cities, as also in Carlisle, and a few other cities in England, complete registrations have been kept for limited periods of time—to serve certain ends—it is evident that as a rule, the conclusions drawn from them are only problematical.

The famous Carlisle life-table was constructed after a period of nine years' observation only, by Dr. Heysham, ending with 1787.†

By this table it appears that during the period in which these observations were made, out of 10,000 children born, 3,203 died under five years of age, and the remaining 6,797 completed their fifth year. Also, that out of 6,797 children who attained to five years of age, 6,460 survived their tenth year, and of these 6,460, 6,047 survived twenty-one. Of 5,075 who attained to forty, 3,643 survived their sixtieth year. "There is good reason to believe," observes Dr. Milne (the author of the article referred to), "that the general law of mortality, that is, the average proportion of persons attaining to any one age, who survive any greater age, remains much the same now among the entire mass of the people throughout England, as it was found to be at Carlisle, during the period of these observations, except among children under ten years of age." The annual mortality of England, estimated by the Carlisle table and applied to the whole of England, is one in thirty-six.

The starting point of modern Medical and Vital Statistics in England, originated about thirty-five years ago, with the evidence elicited for the purposes of the "New Poor-Law." *Medical* men were then first authorised to collect evidence on certain social phenomena, said to favor pauperism and vice. The evidence they produced, showed an amount of information on the causes of disease and degradation that was perfectly overwhelming. Medical science all at once, as it were, sprang to the rescue of the people. Disentangling itself from the web with which caprice, credulity, and empiricism had for centuries embarrassed its labors, it displayed its strength by showing the extent, importance, and application of its resources to the social, no less than to the physical well-being of the whole human race. The doctrine that disease was not inevitable, that its physical causes were removable, was new to the people, but they showed a commendable readiness to

* Paris and Fonblanque. † Ency. Brit., vol. iii.—Annuities.

become converts to the new doctrine. In 1836 a registration law was passed. Following this, in 1839, a new inquiry commission was appointed. Every phase of vital statistics was elaborated and presented. Fever districts were mapped out with boundaries no less distinct than the public parks. Death's harvest fields were exhibited by the multitudes of infants coming into the world year after year, apparently for no other purpose but to die off as fast as possible. And the liability to disease on the part of adults was directly the reverse of what the young who chanced to survive required:—the more children the more orphans. Taking a fixed number of parents, the attacks of fever between twenty and thirty years of age fell but little short of the total at all other ages. From twenty to forty was found to be the most susceptible period, and it is precisely during this time that parents are swept away, leaving orphan families to swell the ranks of famine, disease, and crime. In 1845, another commissioner's report was published, going over the same ground, but entering minutely into details—exhibiting striking differences in degree and intensity, though of the same character and quality;—demonstrating that degeneracy *is* degeneracy, no matter where it exists; that the overcrowded rooms in country villages are not less unhealthful and fatal to their occupants than those of densely-populated towns.

The classification of fever patients in hospitals, showed that nine-tenths come from unpaved and undrained districts. Consumption was shown to be not a "national disease," but a disease promoted by national habits.

Old and hitherto deemed to be reliable laws of mortality were examined into, and shown to be erroneous;—even the Carlisle and other standards of life insurance were shown to be wrong in principle, and unreliable. These and other criteria take the proportion of deaths to the population to represent the comparative mortality in different districts. And these proportions are generally given by statists and received by the public, as representing the average ages at death in any community. Hawkins' Vital Statistics,* Milne's Annuities, Finlaison, Price, Sussmilch, and others† adopt this as the true mode, and it seems to have been universally followed. The following abstract from a paper by Edwin Chadwick, Esq., points out the chief errors of this practice:—

"The public errors created and maintained by taking the proportions of deaths as exponents of the average ages of death, or of the chances of life to the population, may be illustrated by reference to the actual experience amongst nearly two millions of the population, or upwards of 45,000 deaths in thirty-two districts—equivalent to as many populous towns.

"The Carlisle table is taken as a standard for the duration of life, to measure the loss of life in the several districts, as that table gives the probability of life from infancy, well ascertained for one town,

* London, 1829. † Ency. Brit.

and nearly coincides with the experience of the annuity offices on the select class of lives insured by them; and with the results which I have obtained from the mortuary registries, showing the average age of death in the county of Hereford. Each of the recognised insurance tables may, however, be used. If the Carlisle table be taken, the chances of life at infancy would be 38·72; by the Chester table, 36·70; by the Northampton, 25·18; by the Montpelier, 25·36; by the Swedish, 39.39; by the experience of Geneva, 40·18. After the attainment of twenty years, these several tables give the chances of life as follows:—Carlisle, 41·46; Chester, 36·48; Northampton, 33·43; Montpelier, 37·99; Swedish, 38·98; Geneva experience, 37·67; experience of Equitable Society, 41·67. For civic purposes, the most important period for considering the chances of life is after coming of age; the average ages of all who die above twenty-one are therefore given, to illustrate the extent of loss of life to each class of adults, which is the more important to be observed, as it has been hastily supposed that the pressure of the more common and removable causes of disease is almost exclusively upon the infant population. . . . The erroneous conclusions as to the ages of the populations from the proportions of deaths, have, perhaps, arisen from assumptions of the existence of stationary populations, and stationary causes of death. I have been asked, 'If one out of forty dies yearly, must not the average age of all who die be forty years?' The answer by actual experience is, that it is often not thirty years; and perhaps the reason why it is not so, will be most conveniently illustrated by hypothetical cases. For example, let it be assumed, that in any given year forty persons die out of 1,600, which is in the proportion of one to forty, and in consequence of an unusual prevalence of measles, or some disease to which children are subject, the greater number of deaths occur among the infant portion of the population; and hence, out of forty deaths, twenty occur at five years of age, ten at twenty-five, and ten at sixty. Then the total existence had would have been $(20\times5)+(10\times25)+(10\times60)=100+250+600=950$ years, and this divided by 40, the number who died, would give $\frac{9\,5\,0}{4\,0}=24$ years nearly, as the average duration of life to each of the forty who died.

"On the other hand, suppose a severe winter, in which the peculiar causes of mortality may have pressed unusually heavily upon the older lives, and let the numbers who died have been twenty at sixty years of age; ten at forty; ten at five; in such case the total existence would have been $(20\times60)+(10\times40)+(10\times5)=1,200+40+50=1,650$ years, which, divided by 40, would give $\frac{1\,6\,5\,0}{4\,0}=41\frac{1}{4}$ years as the average duration of life to each.

"When these examples are considered, it will be understood that the average of death may remain stationary, or may go on increasing, while the proportions of death remain the same, or vary. The actual mortality of most districts is found to be coincident chiefly with a physical condition, and is most accurately measured

by the years of vitality which have been enjoyed, *i.e.* by the average age of death, or the total number of years which every individual who has died has lived, divided by the number who have died. The number of deaths increase or diminish considerably, and frequently create erroneous impressions, whilst the average ages of death are found to maintain a comparatively steady course, always nearest to the actual condition of the population, and give the most sure indications.

"The chief test of the pressure of the causes of mortality is, then, the duration of life in years; and whatever age may be taken as the standard of the natural age, or the average age of the individual in any community, may be taken to judge what are the standard numbers of deaths in that same community." *

The average age of the living population, calculated according to the Carlisle table, is, to the dying, about as 3 to 4;—the average age of the dying being $38\frac{3}{10}$, and of the living $32\frac{9}{10}$.

In the United States such errors as those referred to by Mr. Chadwick obtain to a still greater degree. Take, for example, the New York City Inspector's Report for 1865, 24,579 deaths are reported from all causes—excepting only those from premature birth and malformation. In order to make it appear that the average duration of human life in New York is as great as in other places, the population is estimated 1,100,000, and as this sum, divided by 24,579, the number of deaths, gives 44·75, the mortality is estimated to be one death in 44·75 of the population—and the average age of the people of New York, to be forty-four years and nine months. By reckoning the population as per last preceding census, 726,683, the average duration of life, by the same rule, would be twenty-nine years and two months. But if, by Mr. Chadwick's method, we estimate the average duration of life by the average age of the decedents, the average duration of life in New York is only nineteen years, one month, and ten days; this being the average age of those who died in 1865, which, according to the inspector, was an unusually healthy year.

In Providence, R. I., where the registration is kept with great care, Dr. Snow, in his report for 1865, estimated the average age of decedents to be, for persons of American birth, 31·10; of foreign birth, 20·67. The average for ten years is, for the former, 31·25, for the latter, 19·63. The proportion of deaths to population, by census, in 1865, was 1 in 45·08. The writer is aware that exception is taken to the average age of decedents, as an index of the average duration of life. But after carefully considering all of the other means accessible to him, he has become convinced that it is the least fallacious, and that the objections urged against it in this country especially—the fluctuations and increase of population—apply with still greater force to the means more usually adopted. To arrive at exact certainty, it is of course necessary to follow up the history of every death, from birth;

* Edwin Chadwick, Esq., London Statistical Journal, 1844.

but this necessity is equally applicable to other means. To estimate the probabilities of life in a community of new settlers by their average, or mean age, would make it appear that such communities are always more healthy than those that are older. And the same objection applies, if we estimate the average age by the ratio of deaths to the living. The fact is, that registration as hitherto practised, with few exceptions, at best only gives approximate results. Exact registration only, such as that practised recently in a few European communities, and aimed at by the Registrar of Vital Statistics of the Metropolitan Board of Health of New York, can ever approach unto the much-desired certainty of the duration of human life in this or any other community.

Nevertheless, the accumulated mortality statistics of the past are far from being valueless. Much information can be gathered from them for a variety of useful purposes.

For example, the influence of season as an index of the effect of climate may be illustrated by the mortality statistics of New York. To this end, the months should be classified according to temperature,* as by the following table:

According to these data, nearly one-seventh of the whole number of deaths in New York are by consumption; and the deaths from this disease are uniformly most numerous in the variable temperatures of April, November, and March, and next, in the coldest weather. Bronchitis differs from consumption only in being most prevalent in the coldest months, instead of the variable; like consumption, it is least prevalent in the hottest months. Pneumonia follows the same rule as bronchitis. Croup, the same as consumption. Erysipelas is less prevalent in the mild months of May, September, and October, than in the hot months of July, August, and June; but it is most prevalent in the coldest months. Scarlet fever is alsol ess prevalent in the mild months of May, September, and October, than in the hot months; but it is twice as prevalent in winter as in summer. Measles, on the other hand, is the most prevalent in the hottest months of summer, and hooping-cough also, but not to the same degree. Diarrhœa, dysentery, cholera infantum, marasmus, and infantile convulsions, it is scarcely worth while to remark, increase, and prevail most extensively with the highest temperature. These five diseases carry off nearly one-fourth of the whole mortality; or, after deducting consumption, nearly one-third of the remainder.

Typhoid and typhus fevers are both recorded as being most prevalent in the temperate months of May, September, and October, but to such a degree only as to lead to the inference that in New York they are not influenced by season.

Other diseases appear to be so little subject to the influence of season as not to require particular notice.

* Deduced from the Meteorological Register, kept at the Eastern Dispensary.—City Inspector's Report.

Effect of Season on the mortality from certain diseases in New York, from 1855 *to* 1865, *the total mortality being estimated at* 248,951.

Diseases.	July.	Aug.	June.	Hot months.	May.	Sept.	Oct.	Temp. months.	April.	Nov.	March	Var'ble months.	Dec.	Feby.	Jany.	Cold months.
Consumption	2577	2785	2400	7762	2879	2785	2888	8552	3021	3102	3132	9255	2687	2968	3072	8727
Bronchitis	262	206	229	697	301	218	291	810	405	413	450	1268	448	433	525	1406
Pneumonia	702	684	811	2197	1206	731	998	2935	1444	1291	1627	4362	1432	1582	1703	4717
Croup	290	266	316	872	480	372	549	1401	625	737	668	2030	558	634	790	1982
Erysipelas	81	73	97	251	153	52	60	265	196	53	97	346	131	195	127	453
Scarlet-Fever	692	480	874	2046	1128	386	483	1997	1338	913	1452	3703	1157	1315	1531	4003
Hooping-Cough	196	407	138	741	155	236	269	750	230	138	253	621	176	227	220	623
Measles	364	358	344	1066	307	157	94	558	305	128	245	678	169	278	276	723
Fever, Typhoid	273	326	252	851	211	359	374	944	219	358	263	840	287	250	258	795
Fever, Typhus	211	272	247	730	302	291	287	880	293	259	251	703	231	302	291	824
Diarrhœa	1221	1687	472	3380	209	1035	578	1822	207	279	204	690	220	200	247	677
Dysentery	587	931	220	1738	108	698	468	1274	122	190	98	410	118	123	135	376
Cholera Infantum	4846	5983	1064	11893	94	2266	579	2939	68	64	41	173	35	28	25	88
Marasmus	1960	2888	1173	6021	992	2379	1696	5067	1027	1070	1105	3192	810	1001	981	2792
Infantile Convulsions	2148	1962	1463	5573	1356	2266	1335	4957	1407	1155	1592	4154	1346	1524	1651	4521

The next most potent influence on the mortality of New York is *age*. It has already been stated that the average of the decedents of New York is only nineteen years, one month, and ten days. Of the whole number of deaths in New York for the last ten years, one-fourth were under one year of age, one-third under two, and more than half under twenty. And if we estimate the average number of births per annum for the last ten years at 25,000, which, according to the ratio of population given by Dr. Harris,* is very near the number, over one-third, including the still births, die under one year of age, and more than half under seven. The remainder have an expectation of life of about forty years.

The predisposing effect of age to particular diseases is most especially manifest in the terribly fatal diseases of infancy, already referred to.

Consumption is least prevalent from ten to fifteen, from which age it increases up to thirty-five, when it is most prevalent, and subsequently steadily lessens.

Bronchitis is most prevalent under one year; after which it les sens up to twenty-five, and then increases till sixty, at which time next to under one year, it is most prevalent. Cancer gives the most deaths between forty and fifty.

Erysipelas is most prevalent under one year, after which it steadily lessens up to twenty-five; it then increases to forty, when it again steadily declines.

Typhus fever, most die of, from thirty to forty, up to which age it steadily increases from infancy.

Typhoid fever follows the same rule as typhus. According to Dr. A. S. Thompson's Statistics in Edinburgh,† the *susceptibility* to fever is greatest under ten, and from twenty to thirty. The sexes are equally susceptible. It is most fatal from forty to fifty. The *fatality* is twice as great in males as in females. Of all attacked, one in fifteen is fatal.

Severity increases 37 per cent., for every decennial advance in age. It is one-third more intense among males than females. Most *prevalent*, July to December, particularly during rainy weather and high temperature. Most *intense*, in dry weather and low temperature. Medical treatment powerfully lessens the danger; early treatment shortens the duration. Mean duration, shorter under forty. Prognosis favorable conversely as the age,—being nearly twice as fatal at forty-one as at twenty-one. Though more intense among males than females, prognosis is one-third more favorable. More favorable from June to December than January to June. One-half more favorable for those who come under treatment early. The second week is the most dangerous. Out of 1,000 passing through second week, eighty-two die.

* Report of the Metropolitan Board of Health, 1866, p. 94.
† Edinburgh Med. and Surg'l J'l.

Sickness-rate. In all communities having a high rate of mortality, the standard of health is usually lessened in a still greater degree, the ratio of persons constantly sick seeming to increase with greater rapidity than the ratio of mortality. In ordinary healthy communities, in England, with a ratio of mortality of about one in thirty-eight, there is *one death to twenty-eight cases of sickness.** In New York, the estimate by the Citizens' Association is, that for every death there are from twenty-five to thirty cases of sickness.†

Death-rate. "In certain parts of England and Wales it is found that, with a near approach to certainty, you may predict that from thirteen to seventeen persons will die annually out of each 1,000 of the population. In certain other parts or districts of that country, it appears to be equally certain that the deaths will exceed twenty in 1,000; in a few districts they will very nearly approach or even exceed 30 in 1,000—the variations from year to year being in this case much more considerable than in the first case referred to."‡ *One* death to *fifty-eight* of the population may, therefore, be taken as a standard death-rate; the excess of this ratio being ordinarily preventable.

In the Fifteenth Ward of the City of New York, which is densely populated, though not much encroached upon by tenement houses, the ratio of mortality to the living in 1863 was one in sixty; in the Sixth Ward, for the same year, it was one in twenty-four. "The important question now recurs: What is the necessary or inevitable mortality of the total population of the city? To this inquiry we need present no other reply than that afforded by the vital statistics of the crowded and busy Fifteenth Ward. Good sanitary regulations would bring up the health-standard in some wards, the Twenty-first, or Murray Hill District, for example, to a point considerably higher than that of the Fifteenth. The entire city, with a death-rate of one in sixty, which is the present rate of mortality in a central, crowded, but salubrious ward, the total mortality of the city would be at the rate of but 16,666 to the million, instead of ranging as it now does from 25,500 to 35,000 deaths to the million of the inhabitants."§ New York, therefore, is evidently capable of being raised to the standard death-rate of the healthiest places in England—one death to fifty-eight of the living.

Climatology and Geography. The influence of the seasons in temperate climates, is, as above noticed, to a considerable degree indicative of the influence of climate. Certain diseases, however, are so completely under the influence of physical climate, that they are susceptible of being arranged into zones of geographical distribution. Such, for example, are yellow fever, plague, cholera, typhus and typhoid fever. And, with rare exceptions, they never

* Commission's Report—Playfair. † Sanitary Condition of New York.
‡ Lectures on Public Health, Gairdner, Edinburgh.
§ Sanitary Condition of New York.

originate in, or visit other places than those which bear the closest resemblance to their original habitat. But, besides this, the laws of climate show that the races of mankind have their prescribed limits of healthfulness. Recent mortality statistics of the British army in various climates,* fully demonstrate that the long-entertained dogma of acclimation is a fallacy. Long residence in a foreign climate affords no immunity from the diseases incident thereto. On the contrary, such a climate acts with redoubled energy on the stranger thereto at every period of his residence, the mortality being greatest among those who remain longest. The constitution may be so modified that comparative health may be retained, yet there is a *morbid degradation* of the physical and intellectual constitution. The European in the Antilles struggles with existence, a prey to fever and dysentery. He is unequal to labor, becomes wasted and wan, and finally perishes.

Of the negro race, the converse is true. The ratio of mortality by Dr. Jarvis' † statistics of mortality in a number of large cities, from Boston to New Orleans, shows the rate of mortality to be among the whites, 2·75; and among the blacks 3·47; this difference being greatest in the North, and finally vanishing in the South; while in the West Indies the difference is reversed, the average longevity being greatest among the blacks. There is also, according to the United States Census Report, a difference in predisposition of the two races to certain diseases.

Asiatic cholera is most fatal to the blacks; but cholera infantum and cholera morbus, diarrhœa and dysentery, and generally the diseases of the digestive organs, erysipelas, intermittent, remittent, typhus, yellow, and scarlet fevers, and small-pox, are more fatal to the whites. The blacks suffer more from asthma, bronchitis, pleurisy, pneumonia, and most diseases of the lungs except consumption—of which there is an excess in the whites. From all diseases of the brain and nervous system, except *tetanus*, the whites suffer most. Scrofula destroys a large proportion of blacks, but tabes mesenterica is more prevalent among the whites.

Of mulattoes in the United States, the ratio of deaths from all causes is intermediate between that of the whites and blacks. The totals from all causes, in a stated number, being

	4,111	whites,	4,511	mulattoes,	5,021	blacks.
From consumption,	1,299	"	974	"	674	"
" scrofula, . . .	51	"	110	"	144	"

In presenting these conclusions from a mass of mortality statistics, all more or less incomplete and imperfect, I am inclined to leave the deductions to each individual interpreter for application. But there is one subject to which I venture to call special attention, namely, the *cause* of the excessive mortality among infants. Cholera infantum, marasmus, and infantile convulsions,

* Statistical Reports of the Army Medical Department—Balfour, Johnson, Macpherson, and others; Aitkin.

† Mortality of the United States, Census Report, 1860, p. 280.

are *the* diseases of all others, which, next to consumption, swell our mortality tables. The children most subject to these diseases are to be found among those mothers who are deprived of fresh air, light, and sufficient food. While by no means intending to lessen the importance of an abundant supply of fresh air and light, as being essential to health, I am nevertheless fully persuaded, after long reflection on the subject, that the want of proper food is more prolific as a cause of these diseases than both of the other usually-accepted causes combined. For after making full allowance for the want of air and light, the experience of this Academy may be safely challenged to the not uncommon occurrence of these diseases, where there is no want of these elements. And especially is this the case among mothers who make child-bed a special penance of water-gruel and tea, and who are so extremely afraid of over-eating in child-bed, that dyspepsia, or indigestion in some other form—rendering their milk unwholesome—is among the most frequent of all causes of their own ill-health, as well as that of their offspring. If such be the case among this class of mothers, to how much greater degree is it the case among those who have this cause in addition to the want of air and light!

But insufficient nutrition is most of all noticeable at the time of weaning. Infants that have healthy nursing mothers, habitually take, during the first year of life, not less than a quart of wholesome milk daily. On weaning, they are usually reduced to less than half of this quantity; and at night-time especially, instead of a full breast of milk to last through the night, or a second meal in the course of it, a cup of weak tea, with a little milk and cracker in it, is the usual substitute. Is it any wonder that children thus poorly nourished, speedily fall into tubercular degeneracy, commonly called marasmus; or that they die of convulsions while passing through the ordeal of dentition?

Dr. Griscom thought that in enumerating the mortality of the different sections of this city, Dr. Bell had made one omission in alluding to the mortality of the several wards; there is also the same omission in the City Inspector's Reports; that the deaths occurring in the different hospitals are put down to the hospital, and not to the wards to which they belonged, which makes a striking difference in the mortality of the wards; hence that distinction should be made. Dr. Bell had noticed this in a foot-note to his paper.

Dr. Harris stated, that for once he was very much gratified to have heard a paper containing so much valuable information, and thought it a document well worthy of a permanent place in the archives of the Academy, and moved a vote of thanks be offered to Dr. Bell for his great favor, which was unanimously carried. Dr. H. said that it was very desirable that physicians, in making out death certificates, should state the exact diagnosis, the cause of death being a matter of such importance, that, if phy-

sicians would endeavor to give it (as it seemed to him most of the members of the Academy and physicians generally did), would render to the public record the exact statement scientifically, would assume from year to year important value, which could be appreciated by the profession, and from which stores of rich information could be drawn. He said it was desirable also that the period of illness should be stated, as it was wished that as complete a record as possible should be kept. He knew of a few public records only which gave any more than the one chief cause of death.

Various questions were asked Dr. H. in regard to coroners' inquests, the duties of the coroners, and what power the Board of Health had over the coroners. The doctor stated that the laws relative to coroners were very loose, and capable of being misconstrued; but he was quite sure that they would not be allowed in matters of doubt to have full control.

STATED MEETING, JUNE 19, 1867. DR. A. C. POST, PRESIDENT, IN THE CHAIR.

Drs. G. H. Humphries and Chas. E. Hackley were inaugurated.

DEATH OF DR. S. POMEROY WHITE.

DR. VAN KLEEK announced the death of Dr. S. Pomeroy White, and offered the following resolutions, which were adopted:

Resolved, That the New York Academy of Medicine learned with profound regret of the death of one of its first, its oldest, and most esteemed fellows, Dr. S. Pomeroy White, who died on the 6th day of June instant, of typhus fever.

Resolved, That the Academy bears testimony to the lustre which in early life Dr. White shed upon American surgery; as well as to a long life in our midst of useful and honorable professional career.

Resolved, That the profession has lost a valued and useful member, the community an honorable and upright man, his family a fond and devoted head; and we desire to express our sympathies in this their bereavement.

Resolved, That a copy of these resolutions, signed by the President and Secretary, be furnished the family of the deceased.

DEATH OF DR. BENJAMIN OGDEN.

Dr. VAN KLEEK also announced the death of Dr. Benjamin Ogden, and offered the following resolutions, which were adopted:

Resolved, That the Academy hears to-night, with heartfelt sorrow, of the death of one of its original founders, Dr. Benjamin Ogden, who died yesterday, June 18.

Resolved, That in Dr. Ogden we recognize one who has adorned and beautified his profession by nearly half a century of usefulness in this, the city of his birth; nor was the knowledge of his worth confined to its narrow limits; for the brightness and goodness of his character, and his reputation in his specialty as an alienist, was coextensive with our country.

Resolved, That he, who was so foremost in every good word and work, deserves our warmest esteem, and should excite and stimulate our earnest emulation.

Resolved, That the Academy will attend the funeral of Dr. Ogden, which will take place from the St. James Lutheran Church, in Fifteenth street, between Third and Second avenues, on Thursday [to-morrow] at 2 P. M.

EXCISION OF THE ENTIRE OS CALCIS FOR CARIES.

Dr. F. A. Burrall exhibited a patient, a boy eight years of age, the subject of an operation performed by Dr. B., on the 1st of November last, for caries, in which the entire os calcis was removed, caused by standing a long time upon the ice nine months previous. The doctor said, that on the 13th of November the boy was able to go down stairs, the leg being suspended, and on December 18 was able to walk with a slight limp, but now has very good use of the foot. A full account of the case will be found in the *Medical Record* for June 15, 1867.

Dr. Post stated, that it was remarkable that there was so much firmness in the foot, the boy being able to walk with much ease.

Dr. Brown then read a paper on "Insanity," by Dr. Ray. A vote of thanks was offered to Dr. Ray, and unanimously carried; after which the Academy adjourned.

Stated Meeting, July 3d, 1867, Dr. A. C. Post, President, in the Chair.

ELECTION OF NEW MEMBERS.

Drs. A. B. Ball, George Bayles, and Charles F. Taylor were elected Resident Fellows.

THE MEDICAL USE OF ELECTRICITY.

Dr. Geo. M. Beard, by invitation, said:

Mr. President: — The attitude in which the profession of America now stand in regard to electro-therapeutics is, to say the least, unfavorable. While there are very few who have practised "localized electrization" with something of the zeal that it is now being investigated in Europe, the vast majority of our educated and reliable physicians have resigned this department entirely to

the tender mercies of ignorant and oftentimes unprincipled outsiders. By this I do not mean that very many in the profession have not essayed and do not occasionally recommend the employment of electricity. There are indeed very few who have not at one time or another had opportunities for testing the power of some form of electricity in the treatment of disease. But if such were asked what kind of apparatus they had recommended, or seen used, they would point to some rusty magneto-electric machine, where the electricity is generated by turning a crank (the ends of a wire being successively and rapidly brought near the poles of a magnet), or to some inconvenient and uncertain electro-magnetic apparatus.

If they were asked in what kind of diseases they regarded electricity as indicated, and for which they had advised its use, they would mention, first of all, long-standing paralysis that had proved rebellious both to time and to all internal medication, and the worst stages of constitutional rheumatism.

If, again, they were asked to describe their method of using electricity, they would confess that in the majority of instances the applications were left to the patients themselves, or more likely to some friend or servant who happened to be nearest; nay more, that the patients were often allowed to hold the positive pole in one hand and the negative in the other, without regard to the locality of the disease.

And finally, if they were asked to give the course and results of this treatment, they would probably reply that after a few applications, made with a weak and uncertain current in the manner above described, the apparatus was laid aside, and as the patient was not cured, or in any way benefited, electricity was afterward declared a humbug by all the parties concerned. You will, I think, agree that this picture of the experience of the majority of those among us who have used electricity is not overdrawn.

There are, it is true, a limited number to whom these criticisms will not apply, at least in their entirety, who have used electricity in some form or other, with a measure of skill and perseverance, though probably not with the success they desired and perhaps hoped for. But in what I say to-night, I shall go upon the supposition that none of you have given this subject any systematic attention, and accordingly, so far as the limited time will permit, I shall enter into the explanations that to an expert would appear very elementary; and inasmuch as the method of *general electrization with the Faradaic current*, that is to be the leading idea in this presentation, was, so far as I know, first formally introduced to the profession of this city by my associate, Dr. Rockwell, and myself, my remarks will necessarily be more or less personal.

BRIEF SKETCH OF THE HISTORY OF ELECTRO-THERAPEUTICS.

The first recorded experiments with electricity on the human body date back over a century. In 1745 a Mr. Von Kleest first received a shock through the system from a Leyden jar. In 1748 a small treatise on the "Effects of Electricity on the Living Human Body" was published by Mr. Jallabert.

The physiological and therapeutical uses of electricity were subsequently investigated by Abbé Sans, Galvani, Grapengiesser, Alexander Von Humboldt, Aldini, Volta, Magendie, Marianini, Matteucci, Du Bois Reymond, Sarlandiere, and Golding Bird. The discovery of the induction current by the great Faraday in 1831 gave a new impulse to investigations in this department, and in 1855 Duchenne, of Boulogne, published a work on "*Electrisation localisée.*" Duchenne may be justly regarded as the father of the modern school in electro-therapeutics, for it was by his writings that the late lamented Remak, of Berlin, was inspired to give this subject special attention. In regard to the currents employed, Duchenne and Remak were diametrically opposed — the former advocating the almost exclusive use of Faradaization, while the latter contended for the use of the galvanic or primary stream from a large number of batteries.

The contention waxed warm between them, and they became the leaders of opposite schools. Moreover, Remak and his followers made their applications chiefly over the motor nerves, at the border points where they entered the muscles, while Duchenne placed the electrodes over the bellies of the muscles in which he desired to produce contraction.

Within the past ten years the cause of electro-therapeutics has enlisted the services of some of the ablest minds in Europe, among whom may be mentioned Becquerel, Rosenthal, Benedict Meyer, Althaus, and Ziemssen, and it also received the very hearty coöperation of the late Prof. Trousseau. In Germany especially, electro-therapeutics has elicited very general attention, and has been made the subject of a variety of publications.

MY OWN EXPERIENCE.

About ten years ago, while pursuing my studies, I began to experiment with the Faradaic current, at first on my own person, and afterward on my acquaintances. The electro-magnetic machine that I employed was to the last degree uncertain and inconvenient. The electricity was generated in an ordinary zinc and copper battery, but the current was at one time quite powerful, at another very weak, and sometimes it would not run at all. My method, as subsequently ascertained, was very similar to that practised by Duchenne and the other European electricians, namely, "localized electrization," the electrodes being placed near to each other, over the parts desired to be affected.

During my college course, and afterward while serving as acting medical cadet in the military hospital in New Haven, I continued to experiment with my apparatus in the same irregular manner in cases of indigestion, nervousness, and rheumatism. Sometimes I placed the negative pole at the tip of the coccyx, and applied the positive over the stomach and liver. The results were pleasing, but not entirely satisfactory. Thus applied, the Faradaic current had a temporarily soothing and strengthening effect, but no very decided or permanent tonic influence.

Rheumatism of the sub-acute and chronic variety, when treated with a mild current through the affected joints, was temporarily benefited, but relapsed so early that there was little encouragement to persevere.

In 1866, Dr. A. D. Rockwell and myself associated ourselves together for the purpose of giving special attention to electro-therapeutics, and we soon began to meet with results which before neither of us had even anticipated, for the reason that we used better instruments and a different mode of application.

In the first place we secured a neat, convenient, and reliable electro-magnetic machine, such as I now show you. It is manufactured by Jerome Kidder, and is decidedly superior for medical use to any other form of electro-magnetic apparatus that I have ever seen. For office use it leaves nothing to be desired. It is run by the ordinary Smee's battery (zinc and platina), or by Farmer's thermo-electric battery, and has a powerful helix, as well as a readily-adjusted armature.

In the brief space allowed me to-night there would not be time to enter into detailed explanation of the various parts of this beautiful machine, but I shall confine my remarks, as far as possible, to what is of direct practical import.

In the treatises devoted to this subject you will read of "Galvanization" and "Faradaization," "quantity" and "intensity," "direct" and "inverse" currents, and other equally arbitrary terms that can but bewilder those who have not given the subject particular attention.

EXPLANATION OF TERMS.*

It should be remarked that among medical writers in general, much confusion still continues to exist in the use of these terms. "Battery" and "electro-magnetic machine," "Galvanism" and "Faradaization," "primary" and "secondary," are often confounded, even by those who in nearly every other department of medical science are well informed.

GALVANIC BATTERY — ELECTRO-MAGNETIC MACHINE.

A *galvanic battery* is usually formed of three substances, two of which are metal, and the other some dilute acid that generates

* This Explanation of Terms is mainly taken from our recently published work on the Medical Use of Electricity.

electricity by its action on one of the metals. It is also called an "element" or "cell."

The *compound galvanic battery* consists of a number of simple batteries connected in a series. Thus we have the voltaic pile, and Grove's, Bunsen's, or Smee's compound battery, composed of more or less simple batteries of their respective varieties.

Farmer's thermo-electric battery, composed of bars of German silver, and an alloy of zinc and antimony, arranged in a V shape, so as to be heated (by gas or an alcohol lamp) at their point of union, has recently come into use.

An *electro-magnetic machine* is composed of a helix and vibrating armature, connected with some form of battery. It may be used with Bunsen's, Smee's, or Grove's battery, or with any modification of them, or with Farmer's thermo-electric battery.

In *magneto-electric machines* a soft iron armature is made to pass rapidly the poles of a magnet by turning a crank. They are very inconvenient, and must soon give way to electro-magnetic machines.

QUANTITY — INTENSITY, OR TENSION.

The *quantity* of electricity generated by any apparatus is proportioned to the amount of chemical action that takes place in the battery. Intensity is that power that enables it to overcome resistances that may impede the progress of the current.

The electricity of an ordinary thunder-storm is great in intensity, but exceedingly small in quantity. On the other hand, that which is generated by any ordinary form of battery has comparatively a great deal of quantity, but little intensity. The *quantity* supplied by any battery may be increased by increasing the chemical action that takes place in it, either by making it larger, or by strengthening the acid solution.

In a *compound battery* the *intensity* is increased in proportion to the number of batteries, but, as they are ordinarily united, the quantity is no greater than in the single battery of which it is composed.

The relative meaning of these terms may be best understood by an illustration. A gallon of water heated to 100° has a much greater quantity of heat than a pint heated to 500°; but the heat of the latter is much more intense.

GALVANIZATION — FARADAIZATION.*

The current that proceeds from any simple or compound battery is called the "primary current," sometimes also the "continuous," "constant," or "uninterrupted" stream. Its use in medicine is called *galvanization*, from Galvani.

* We have adopted the orthography *faradaization* in preference to *faradisation*, introduced by Duchenne, for the reason that, while both are equally legitimate, the former has the additional advantage of more directly suggesting the name *Faraday*, from whom it was derived.

When the current from any simple or compound battery passes through a helix and induces another current that is interrupted by a vibrating armature, such a current is called the "induced," "secondary," "interrupted," or "faradaic," from Faraday, and the entire apparatus is called an electro-magnetic machine; its use in medicine is called faradaization. This is the current that we usually employ in *general electrization*. In Germany, the current from a voltaic pile is called "continuous," that from all other batteries "constant;" with us this distinction is not ordinarily observed.

ASCENDING — DESCENDING.

The course of an electric current is always from the positive to the negative pole. When, therefore, the current runs from the periphery toward the centre, against the course of the nerves, or of any single nerve, it is called ascending, or "reverse;" when it runs from the centre toward the periphery, with the course of the nerves, or of any single nerve, it is called the descending or "direct" current.

The ascending current in its immediate effects is painful and irritating; the descending is calmative and exhilarating.

In general electrization we use mainly the descending current; in localized electrization either the ascending or descending may be used, according as exciting or soothing effects are indicated.

GENERAL ELECTRIZATION — LOCALIZED ELECTRIZATION.

In *general electrization one of the electrodes is placed at the feet, while the other is applied over the entire surface of the body, and the whole system is affected by the current.*

In *localized electrization* the electrodes are applied near to each other, and the effects of the current are supposed to be mainly confined to the tissues lying between them.

The method of applying electricity which we have chiefly adopted I have denominated *general electrization*, in distinction from the localized electrization of Duchenne.

The *modus operandi* of general electrization is as follows: Male patients remove their stockings, and all their outer and under clothing from the upper part of the body, and place their feet on the piece of copper to which the negative pole is attached, while the operator applies the positive, either a wet sponge or the moistened hand (when the current passes through his own person), down the spine and over the vital organs. Ladies remove their dress and loosen their under garments, and throw over their shoulders a shawl, or sheet, to prevent exposure. The most thorough form of application demands that *the entire surface of the body* should be gone over, with some regard to order.

It is manifest that when electricity is applied in this manner, the currents must traverse the whole system, and consequently

all the vital organs as well as the various muscles and nerves of the extremities are directly acted upon. *From the astonishing results of this method of treatment also it would seem that the great sympathetic are powerfully affected.*

The *immediate* effects of general electrization, when carefully, skilfully, and thoroughly applied, are a feeling of exhilaration, very much as is experienced after taking a bath, or after a brisk walk in the open air. If there be any distress in the head, or neuralgia or rheumatism in any part of the body, or severe pain from any form of inflammation, acute, sub-acute, or chronic, it is not unfrequently entirely dissipated, and sometimes there may be a grateful somnolence. The *secondary effects*, that do not usually appear until the following day, vary in different individuals, and in different states of the system, and also according to the condition of the atmosphere. At the outset of a course of treatment, patients sometimes complain of a feeling of depression, and of soreness in the muscles, especially if the applications have been too powerful, or too long continued. But these unpleasant sensations are exceptional, rather than the rule, and the *permanent constitutional tonic* effects begin to manifest themselves, progressing sometimes with wonderful rapidity, and in other cases with slow and steady improvement. The capricious appetite becomes keener and stronger, the constipated bowels grow more regular, and the sleep is easier and more refreshing. The muscles increase in size and hardness, and there is greater capacity of endurance. The spirits rise as always with the general condition, and the hypochondriac forgets his imagined woes. As would be expected, various special diseases, that depend on, or are associated with constitutional debility, are very often relieved, and in many cases are approximately or absolutely cured by continuous treatment. We have employed general electrization in cases of neuralgia, the various forms and complications of indigestion, rheumatism of the acute, sub-acute, and chronic types, paralysis of the functional, rheumatic, traumatic, hysterical varieties, and in many other cases not dependent on a persistent, active, and incurable central lesion, chorea, hysteria, anæmia, dysmenorrhœa, amenorrhœa, and, indeed, in nearly every shape of disease associated with constitutional feebleness, except pulmonary tuberculosis.

I lay especial stress on the use of the hand as an electrode, because I was long since abundantly satisfied that it was more easily controlled, more effective, and far more agreeable to the patient, than any metal, or even the softest sponge. Applications to the head, or over any particularly sensitive organ, should always be made with the hand. Those who attempt to apply electricity in this way will find it necessary to practise extensively, in order that they may get so accustomed to the passage of a powerful current through their own persons, that they can manipulate without difficulty all the parts desired to be affected.

The question at once arises, what effects must the operator

experience who thus allows a strong current to pass through his own body to the patient? To this interrogatory only experience can reply. Theoretically it would seem that an agent which we have proved to be so powerful to cure and to save, must also, when used in excess, be equally mighty to destroy.

Duchenne nor Remak did not advise this method, although they suggested its possibility; nor have any of their disciples, at least those who are numbered in the ranks of the profession. Garratt earnestly advises against it, declaring that it could but be injurious. But dismissing theories, let us consult the facts. Wm. Miller of this city, known to many of you as a man of large experience in the application of electricity, and whose practical suggestions have been to us of immense service during all our investigations on the subject of general electrization, has thus allowed an induced current of electricity to pass through his person, on the average, several hours a day for thirty-six years, and has suffered from no affection that could be in any way referred to this cause. When he commenced this practice he was slender and feeble; but under this mighty stimulus he has become quite firm and sturdy, and he can now operate with perfect ease, while there is rushing through him a current that would temporarily paralyze any one not equally accustomed to it.

My own general health has improved since I began to make applications through my own person; indeed it was the calming and invigorating effects experienced by myself that first called my attention to the tonic powers of electricity. My associate, Dr. Rockwell, has also grown somewhat stronger under the same influence, and there has been a very perceptible increase in the size of the muscles of the arms of both of us.

In order to illustrate more specifically the indications for, and the results of general electrization, I will sketch a few cases in as much detail as the time will allow.

Case I.—*Neuralgia, Menorrhagia, with Anæmia.*—Mrs. S., a married lady, aged thirty-five, had for several years suffered from periodical attacks of severe pain in the stomach and head, attended with great prostration. The paroxysms occurred regularly every month during the catamenial flow, and sometimes at intervening periods, though with less severity. To the other distressing symptoms was sometimes added obstinate and persistent vomiting. The stomach was so weak that at times she was obliged to keep her clothing loosened, and the slightest pressure over the epigastric region caused excessive pain. Her menstrual flow was usually quite protracted—from five to seven days in duration—and the quantity of flow lost was very great. Immediately after her courses she would present, for a number of days, a strikingly anæmic appearance. The opinion of her previous medical adviser, that the menorrhagia was a consequence of general debility rather than of any organic uterine disease, was confirmed by our own examination, and accordingly we decided

to employ general applications of the Faradaic current. She had been using internal medication so long, and with such unsatisfactory results, that we yielded to her express desire and gave her no drugs whatever.

The treatment by electrization was commenced December first, 1866, and continued two or three times a week, for two months, with the most unexpected and pleasing results.

She was of a frail, delicate build, and in all parts of the body so susceptible to electrical influence that only the mildest current could be used, and, what is contrary to our usual experience, she could bear no more at the close than at the outset of the treatment.

The first *séance* was followed the next day by considerable nervousness and by soreness of the muscles that very soon passed away.

After two weeks of treatment her courses came on, and were as long continued and attended with as much loss of blood as before. One night, while she was suffering from a paroxysm of neuralgia in the stomach of unusual severity, we applied a mild current over her head and vital organs for about ten minutes, with the effect of entirely relieving her pain, and she soon passed into a quiet and refreshing sleep. On the following day the pain returned, but with much less severity.

Four weeks from that date her courses again appeared, but they were attended with comparatively little pain. Their duration was but four days, and but a little more than the normal quantity of blood was discharged.

The improvement in her general condition during the entire progress of the treatment was very decided; her appetite became keener, her digestion easier, her sleep more refreshing, and her spirits more buoyant. After twenty-four applications, distributed over a period of two months, all her distressing symptoms, the neuralgic attacks in the head and stomach, the menorrhagia, and consequent anæmia, entirely disappeared, and from that time to the present (for we see her frequently) her health has been excellent.

Case II.—*Neuralgia, Leucorrhœa, Chronic Diarrhœa with General Debility.*—Mrs. P., a married lady, thirty-five years of age, of a tall, spare form, first applied to us for electrization in May, 1867. She stated that a protracted and fearful labor, three years before, had so reduced her that she had never been able to regain her strength. She complained of general weakness, poor appetite, and inability to make any exertion. She suffered almost constantly from pain in the back, and from acute neuralgic attacks in the top of the head.

There was a continual bearing down sensation, and a frequent desire to pass water. She had all along suffered more or less from a leucorrhœal discharge, and for about six months before we saw her, she had been growing weaker and weaker from a persistent diarrhœa. On examination per vaginam, we found an indurated

os, the result of chronic inflammation, and slight prolapsus. She was, in a word, a typical case of special and constitutional weakness that we so often meet with among ladies in the better walks of society.

In this, as in the previous case, internal medication in the shape of iron, quinine, strychnine, had been tried by her skilful medical adviser so frequently, and so uselessly, that we resolved to discontinue it altogether.

Even at the first sitting she was able to bear a good strength of the current, except on the top of the head, and over the transverse colon. There were no unpleasant secondary symptoms, and she slowly but steadily improved in her general condition and in her special symptoms. The diarrhœa was checked, and the stools gradually assumed a more healthy appearance; and with this improvement in the condition of the bowels, the tender spots over the transverse colon became less sensitive to the current. On account of the diarrhœa she had been forced to be very scrupulous in her diet, but she could now eat all the ordinary articles of food with impunity. She could sleep freer and longer than before, and had gained very perceptibly in strength and color. Her headaches were the last symptoms to yield, but they diminished in their violence and frequency. The treatment was extended over a period of two months.

Case III.—*Neuralgia, Dysmenorrhœa, and Functional Paralysis.*—Mrs. M., a married lady of twenty-seven years of age, reported that in January, 1867, she was prostrated with an attack of intermittent fever that left her in a very weak, anæmic, and neuralgic condition, from which no combination of internal tonics seemed to enable her to rally. Her symptoms were intense neuralgic pain in the fore and back part of the head, over the ribs, on both sides, and in the left arm. As she complained of some leucorrhœa, and had suffered in times previous from prolapsus, we examined her with the speculum. There was congestion of the cervix, but the uterus was in its normal position. She was thin and very pale, and was unable to attend to her daily tasks. Whole nights she had passed without sleep, and, as a natural and inevitable consequence, her appetite was very weak and capricious, and her bowels were irregular. Her left arm was not only exceedingly painful, but it was also so much affected with functional or reflex paralysis that she could not raise it from her side.

She experienced temporary relief immediately after the first *séance*, but the pain returned on the following day. After the fifth application there began to be evidences of improvement in her general condition, although her neuralgic symptoms were but temporarily abated, and her left arm seemed to be more powerless than before. After she had been under treatment about three weeks, she was attacked in the night with such severe paroxysms of neuralgia in the uterus that her friends became greatly alarmed. Treatment by general electrization was resumed, and

her improvement was very rapid. Her appetite became almost ravenous, and her rest at night was very comfortable, and sometimes unbroken. Her face was fuller, and she increased in weight. There was more expression in her countenance, and her spirits, that had so long been depressed, were again buoyant. *The paralyzed arm was entirely restored.* The treatment was extended over a period of seven weeks. Her menses appeared twice during that time, and were comparatively painless.

In regard to the above and a number of very similar cases treated by general electrization, it should be remarked:

1. The special symptoms, neuralgia, headache, constipation, etc., were dependent on general feebleness.

2. These special symptoms did not ameliorate or depart until the general system began to be strengthened. In a word, the affections were cured by the *tonic* effects of *general electrization.*

3. The temporary relief of severe neuralgia and inflammatory pain, effected by general electrization (and also, when the symptom is confined to a single part, by localized electrization), is oftentimes as speedy and even more absolute than that produced by hypodermic injections. More than that, the former is a permanent tonic, while the latter method is apt to be followed by a depressing reaction, and when too often repeated, may work injuriously on the constitution.

RHEUMATISM.

This disease in all its stages is treated by general electrization, and almost uniformly with more or less permanent benefit.

Mr. H., aged forty-five, was sent to us by Dr. Austin Flint, to be treated for intercostal rheumatism on both sides, that had annoyed him for many months. He had been under Dr. Flint's observation for about a year, during which time he had been taking the iodide and bromide of potassium, with very slow but quite perceptible improvement for his general rheumatic affection, but the difficulty in the side was not disposed to yield.

He was at no time free from a considerable sense of annoyance, and the constant aching often rendered his nights sleepless.

Mr. H. was a stalwart, hearty man, but had suffered at times from rheumatism all his life, and when he came to us seemed to be almost discouraged for fear lest he might be obliged to give up his business. He received general applications with considerable regularity, two or three times a week, for two months, with the result of entirely removing the intercostal soreness, stiffness, and pain. The disease is, of course, still in his system, manifesting itself chiefly in the hips; but general electrization seems to have done more than internal medication. A peculiarity of his case is that he has usually suffered most in warm weather; his improvement under general electrization took place as the summer was coming on.

I cite the above because it is typical of one of the most obsti-

nate cases of rheumatism that we have met with. In a great number of cases that we have treated during the past year, permanent benefit or absolute dissipation of the symptoms has resulted from a short course of treatment, without the aid of internal remedies. A few of these have already been reported in our work on the Medical Use of Electricity.

DYSPEPSIA.

This malady, with its various symptoms and complications, receives from general electrization a benefit proportioned to the severity of the disease and the perseverance of the treatment. Most of the dyspeptics consult us for other and more directly fearful or alarming affections, but often the dyspepsia must first be remedied, before the accompanying maladies show any evidence of yielding.

Where emaciation has resulted from long standing dyspepsia, general electrization may cause the patient to increase in flesh, sometimes quite rapidly, though more commonly after the treatment has been somewhat protracted, or has been suspended.

If general electrization were only useful in cases of neuralgia, rheumatism, and dyspepsia, it would be worthy of the most careful study on the part of all who desire to do the best possible for their suffering patients.

But there are a number of other affections in which general electrization will succeed when internal medication fails, though the results are not as brilliant nor as uniform as in the diseases of which I have just spoken. I refer to *paralysis, chorea, and amenorrhœa.*

The cases of paralysis that are sent to us for treatment may be arranged under three classes.

1. Those that are absolutely incurable.
2. Those that can be greatly or appreciably relieved.
3. Those that can be entirely cured.

The fact of the existence of a central lesion is no ground for discouragement, if it be neither persistent nor progressive. It is oftentimes impossible to determine in regard to the curability of any case of paralysis without actual trial. We are often disappointed, both favorably and the reverse. As a rule, the improvement, even in the fortunate cases, is far from being rapid. The movement cure and general electrization may be advantageously combined in the treatment of the various forms of paralysis in children and in adults. Our experience with paralysis is more fully detailed in our series of articles on the subject in the *Medical Record.*

In *infantile paralysis* we have achieved pleasing results. Indeed I am quite confident that in *this, as in various other paralytic affections, general electrization with the Faradaic current will accomplish much that has been claimed for localized electrization with the primary current.* My reasons for this opinion are based

on our experience on the history of cases that have already been published, and to these I refer.

Chorea, as is well known, will often recover of itself, or under the administration of iron. But sometimes a case defies all internal medication, and even time itself. We have cured a few such, but only after very protracted treatment.

In *amenorrhœa* the effects of general electrization are as uncertain as the causes are vague. The menses are sometimes brought on after a single application, more frequently after a course of treatment. In the case of a married lady who has been under our care for a number of weeks, the courses have just reappeared at the eleventh hour, after more than a year's absence.

Amenorrhœa, chorea, and paralysis are usually associated with general debility, and they do not ordinarily yield to general electrization until the system is first invigorated in all its functions. In local palsies occurring in those otherwise healthy, I use only local applications, and usually with the ascending current.

But although general electrization is a tonic, and fulfils a large variety of indications, it is very far from being a cure-all. It is subject to limitations, as are all other tonic influences, but in many phases of disease it is far more speedily and permanently beneficial than any system of internal medication. The truth of this somewhat bold statement is forced upon us in our daily experience. To tone it down would be to misrepresent.

But in epilepsy, taking the cases as we find them, in very obstinate life-long rheumatism in the aged, in paralysis dependent on persistent central lesions, and in pulmonary tuberculosis, general electrization is generally as powerless as are all the tinctures and powders of the shops.

But these (excepting phthisis) are the very diseases for which electricity is ordinarily advised.

In obscure cases we sometimes feel justified in using electricity empirically, since we are confident that in practised hands it can rarely be harmful, even though it does no good.

In the case of a gentleman affected with facial paralysis and absolute deafness, sent to us by Dr. Clark, we employed local and general electrization for over a month without any benefit. The question whether the affected nerves were yet alive seemed to be answered in the negative, and the treatment was suspended.

By suggestion of Dr. George T. Elliot, I tested the power of general electrization on a young woman in Bellevue Hospital, who had long been suffering from menstrual epilepsy. For a time she seemed to be slightly improved, but after eighteen applications she was not enough better to encourage me to continue the treatment. It should be stated that, before I saw her, she had been previously treated in various ways, and even by electrization through the uterus.

In the employment of general electrization, as in all other

mechanical procedures, there are certain rules and various minor details, on the observance of which the success of the treatment very materially depends.

1. Instead of the ordinary metallic electrodes that accompany Kidder's machine, we always use a hollow brass ball covered with sponge. This sponge should always be wet, and the hand making the application should be repeatedly moistened.

2. In the case of very aged, or very weakly patients, the first application should be very mild and of short duration, in order that there may be no unpleasant secondary effects. A very old and infirm lady, sent to us by my friend Dr. Roosa, expressively summed up her experience, after the first application, by declaring that "she felt all the horrors of the damned."

3. The amount of electricity one can bear depends on the age, health, temperament, habits, and even on the state of the atmosphere. After we have, by experience, ascertained the degree of susceptibility of any patient, we should make the applications as strong as they can well be borne.

4. The duration of the applications should be from five to twenty-five minutes, according to the constitution of the patient, and the nature of the disease; and they should not be repeated oftener than every other day, or two or three times a week. Very old and nervous patients demand longer intervals in the treatment.

5. The applications should be so thorough that every part of the body is traversed by the current. Special pains should be taken to avoid the bony prominences, or, when they are touched, to carefully graduate the current according to their sensitiveness. Careless or slipshod applications will bring uncertain results.

In the majority of chronic affections the treatment should be persistent. Certain acute attacks may be sometimes dispelled by one application, but long-standing cases of any form of disease demand perseverance with general electrization as with any other form of treatment. The cures are achieved by *general* electrization *co-operating with nature and time.*

Considered even as a sketch of the medical uses of electricity, this presentation to-night has been exceedingly imperfect. Of the Voltaic pile and the various compound batteries for the primary current, or of Farmer's neat and convenient thermo-electric apparatus, I shall not have time to speak. Nor can I allude to very many special types and phases of disease in which electrization is found to be of service, either alone, or as an adjuvant to other remedies.

But my object will have been gained if I have succeeded in calling your attention to what I regard as the corner-stone of electro-therapeutics, viz.: that *general electrization with the Faradaic current, as I have described it, is a* TONIC *oftentimes more rapidly and surely effective than any internal medication, and therefore indicated in a large variety of diseases;* that it is to be

used prior to, or in conjunction with, quinine, strychnine, iron, exercise, bathing, and all other tonic influences whatsoever.

It is too much to expect that the stone that has so long been rejected will at once become the head of the corner; but without regard to the special method of application that has been the subject of this paper to-night, I think that any one who considers with attention the history of the medical use of electricity in Europe for the past ten years, will become convinced that the cause of electro-therapeutics is too rich in promise for the future to be any longer resigned to the hands of the Philistines, who have thus far snatched from it whatever of glory or of dignity it ought long since to have acquired under the patronage of our leaders in science.

STATED MEETING, OCT. 2D, 1867. DR. H. D. BULKLEY, 1ST VICE-PRESIDENT, IN THE CHAIR.

Dr. Prince read the following paper:

RHYNCHOSIA EXCAVATA, OR TSA-TSIN.

MR. PRESIDENT AND GENTLEMEN: It is now about two years since the subject of our paper was presented to the Academy by Dr. Garrish as a "new remedy," with a request that experiments might be made in order to test its properties.

The Obstetrical section accordingly was requested to take charge of the matter. At its next regular meeting a committee was formed for the purpose, and reports from time to time were made by members of the committee and other gentlemen. This paper was read to the section last March as the result of these experiments, and is now respectfully submitted:

MR. PRESIDENT: During the last year, in accordance with your request, I have made from month to month a verbal report of the cases in which the drug known as Rhynchosia Excavata, or Tsa-Tsin, had been used, with the view to test its emmenagogue properties. I now propose to present a review of its effects upon some twenty-six cases in my own practice, and also some twenty-nine cases in which the drug has been used in the practice of other well-known physicians, who have kindly reported their experience. I should have been happy to have presented to you the result of our experiments long before this, but the cases were so few, that we could hardly form a proper estimate of its value, and even with those which we have now gathered we may not be able to decide as to its true worth. The remedy thus far in my hands has acted well, and also in the practice of most of the gentlemen who have reported their cases. I would not present this drug as a new remedy, as I find several articles introducing the medicine in the London *Lancet*, and copied by Braithwaite, some sixteen years since, in which the author claims quite as much as we do for it; and although some of his suggestions we do not

endorse, still most of his assertions have proved correct, in the limited number of cases which we beg leave herewith to present. In regard to its history I find but little. I am told by the gentleman who imports the drug to this country, that after diligent search he can find no full treatise on the subject.

Tsa-Tsin is used by German physicians, and I am informed that several articles in regard to the remedy are found in German medical periodicals. We are indebted to Dr. Zinzer for the following translation:

Dear Doctor: The following article I find in several periodicals, viz.: *Journal of the Austrian Apothecary*, 1865, No. 1; also, in the *Ausland Journal*, 1865, from which I give you the following translation:

During many years rumors and short notices of a Chinese remedy for chlorosis and irregularities in menstruation have reached Europe, viz., "Tsa-Tsin;" but no reliable or definite statement has been given, because the Chinese Government is strongly opposed to the export of the Tsa-Tsin, making the same a valuable part of revenue, by exterminating the plant everywhere except in a narrow district in the steppes of the Mandschurei.

In 1854 a German apothecary, Otto Schmidt, visited the Mandschurei, and became acquainted with the Mandarin who had charge of the district, and of the gathering of the Tsa-Tsin. This gentleman informed him of the excellent qualities of the plant, but did not allow him to take away a branch of it for his herbarium. Mr. Schmidt, however, secreted a ripe holly, the seed from which he planted on a small farm in the Russian extent of the Amoor river; and since the fall of 1863 has sent considerable quantities of the plant to Leipsic.

The experiments with the drug are said to have proved that most of the catamenial irregularities can be cured by it. The decoction of the leaves has a nice yellow color, an aromatic smell, a strong taste, and acts as a sedative upon the uterine system. It does not only relieve from all pains, and cramps, and the commencing menstruation; but forwards the excretion itself, and in a few days all symptoms of chlorosis or disturbance disappear.

Tsa-Tsin belongs to the species Rynchosia (Papilionaceæ). Mr. Schmidt has called it "Rynchosia excavata." The plant comes into the market in the form of a coarse-grained powder of a nice light-green color; the leaves immediately after plucking being cut into small pieces, and dried quickly on hot plates (the Chinese way); price 1 Prussian thaler (about 75 cents gold) per ounce; three drachms necessary for one cure.

Kindest regards.

Dr. Zinzer.

The article as an emmenagogue is by no means confined to German practice, as the letter from Dr. Williams above referred

to will prove. The description which he has given of the tree and its uses, is the only one which has met my notice; and in this connection is of so much importance that you will please pardon my reading it verbatim.

From the London Lancet, 1850.

"After twenty years' absence from my native land, during which I have visited in the various islands of the Pacific Ocean and Eastern Archipelago, anxious for information bearing upon my profession, as I have always been, I could scarcely avoid picking up at various times many matters of practical importance, as well as some curious information of the mode of treating diseases, more particularly amongst the Japanese and the Chinese. The result of my inquiries I will, with permission, forward to the *Lancet*.

"Among the many botanical remedies that popular experience has made common in Japan and China, is one that is believed to exert specific influence upon the uterus, more particularly in cases of sterility and checked menstruation; and from the numerous cases in which I have administered it with advantage, I am inclined to think it will become a valuable addition to our materia medica.

"The tree from which the preparation is made that possesses the virtues ascribed to it is, no doubt, one of the order Ternstrœmiaceæ of Jussieu, growing to the size of the English laurel, with leaves somewhat larger than the Congou tea, and botanically it may be described as: leaves lanceolate, alternate, serrate, downy on the underside, and emitting when bruised a strong odor, resembling pulegium, and sabina. Its properties are said to vary considerably with the age of the tree; likewise as to the season of the year. It is perennial, and grows best in moist and sheltered places.

"The mode of preparation is to take a quantity of the leaves, macerate them in as much rice spirits (samshu) as will just moisten them, for six hours; then express, and give about a teaspoonful every hour, and two or three doses will invariably bring on the menstrual secretion, which can be maintained by a dose or two daily for any length of time.

"Females in the healthy state are expected to menstruate in their eighth year in Japan and China, and should they not do so they are ineligible for betrothal; therefore recourse is had to the key-tsa-tsing with certain results.

"To insure its success, according to popular belief, the leaves must be gathered by a virgin, the while using certain cabalistic formulæ, at the full of the moon, and during the burning of a certain number of perfumed joss-sticks.

"When required for the purpose of obviating sterility, the tree must be in its second year, and also gathered, with certain prescribed formulæ, at the wane of the moon, and equal parts of the

root must be added to the preparation, which is made in the same manner as the preceding receipt.

"That the root is aphrodisiac in its effects I have not the slightest doubt, as I have administered it to animals with obvious results, and without any ill effects even to mules and castrati.

"Having brought home some of the plant, and various drawings which I intend sending to the Medico-Botanical and other learned societies, as well as sufficient to insure a reasonable trial of its effects in this country, I shall feel happy to forward some of the preparation to any established member of the profession, on the promise of his giving me the result of the trial, and prescribing it according to the instructions I will forward with it."

Upper Clifton street, Finsbury, 1850, *Lancet*, p. 321.

Communicated by E. Williams, M.D. A short time since you favored me by the insertion of a communication upon the advantages of the Japanese remedy key-tsa-tsin as an emmenagogue. I shall feel obliged by your insertion of the following letter from a physician of the Winchester Hospital, Dr. White, relative to its success in a most interesting and obstinate case. The following is selected from upwards of one hundred others, as the result of my offers to the profession through your paper: Charlotte M— is a stout girl of a florid complexion, twenty years old, single, and lives with her mother. The catamenia appeared at the age of thirteen, but ceased at the age of sixteen, previously to which she had epileptic fits for three months. For three or four years previous to the administration of the remedy, she had had regular attacks of hæmoptysis or epistaxis every three or four weeks, though no sign of tubercle could be detected in the chest. Every means was tried to restore the discharge, but says Dr. White:—

"All having failed, after six months time I resolved to make an experiment with the key-tsa-tsin. On November 6th she commenced taking three drachms of this medicine, every six hours, in a little hot gin and water, and continued to do so for three days, when, not having experienced any effect from it, except some pain in the back, she omitted it for two days. I urged her to persevere with the medicine a little longer, and she re-commenced taking it every four hours. On the second day (Nov. 13th) the catamenia appeared and flowed normally three days; she felt no inconvenience from the medicine, except some slight pain in the back."

Remarks.—I will not occupy your pages by any lengthened remarks. I am justified in concluding from the above, and the numerous other reports I daily receive, as well as from cases in which I have had an opportunity of exhibiting it in my own practice, that this remedy will prove one of the most certain and valuable additions to our materia medica; and the only fear I have in its introduction is, that it may be abused, and become the too ready instrument of procuring abortion. I shall, however, for the

present make such arrangements as will, I trust, prevent its abuse, while its legitimate purposes will meet with no obstacle."

Dec. 1850, *Lancet*, January 1851.

It is somewhat singular that a remedy possessing such power as seems to be in the cases reported by Drs. Williams, White, and the hundred other gentlemen mentioned in his report, should be heard nothing of for the last sixteen years. The only solution that I can give is, that the drug has not been in the market to any great extent, and consequently could not be obtained. There are several points in this letter to which I would particularly call your attention.

1st. That the drug may be used for the purpose of producing abortion, Dr. Williams seems to accept as a fact, without any corroborative experience, whereas the truth is precisely the reverse, as the cases herewith I think will abundantly establish.

2d. There is some difference in the administration of the remedy, also, which I beg you to notice, and which may account for the more violent results, viz., he uses the root in connection with the leaves; he also combines more or less spirits in the preparation. We have thus far used the simple infusion. The exact quantity used by him is not stated, but it was probably larger than that used by us. It is not improbable that if we had used the drug as freely, we should not have to report as many failures as we do.

This statement would have been much more satisfactory, if Dr. Williams had not only mentioned his and others' successes, but had also spoken of the failures which had attended their experiments. Amenorrhœa is dependent upon various diseases and conditions, and consequently there are few medicines, as a class, less to be depended upon than that of emmenagogues; and no matter how efficient any one remedy may be, we must expect failures if applied to all cases; and in the cases in which I have used the drug, I have also combined such general treatment, and regimen, as was calculated to restore the normal condition; and when such condition could not be restored, as in the cases of phthisis, the discharge failed to appear, although large portions of the remedy were used. And I should strongly recommend, while using the Tsa-Tsin, to also combine such other treatment as the symptoms may indicate: viz., if plethoric, before using the remedy have your patient properly depleted, and *vice versa;* when anæmia is a prominent element, resort to tonics and chalybeates; if constipation is present, restore the tone of the digestive organs, and if uterine ulceration or disease exists, endeavor, as far as possible, to remove these abnormal conditions, selecting favorable opportunities for the exhibition of the drug, particularly at the time when we may naturally look for a return of the physiological condition. It may be urged that, if we can effect this object, viz., a restoration to h e normal state, in many instances no further treatment will be required. We readily agree to this proposition; still it does

not in the least detract from the value of the remedy, if, as has been the case thus far, after such regimen or treatment we find it effectual.

One of the greatest recommendations to the use of the Tsa-Tsin has been its harmlessness. Most of the remedies heretofore used are more or less violent in their effects, disturbing or shocking the system; in fact, we have but few remedies that are used as emmenagogues, that act directly upon the organ, or that may be called specific in this relation. Our patients are frequently aware of this fact, and consequently we hear of the most absurd measures being adopted in order to effect their object, such as crawling down-stairs head first, carrying heavy loads, lifting heavy weights, taking drastic purgatives, etc., etc.

One of the most ludicrous pictures was suggested to my mind some time since, by one of our highly respectable and fashionable ladies, who was thirty-six years old, had had several children, and was not anxious to have the number increased. She was rather large and decidedly fat. Her courses not having come to time, she became alarmed, and, as she informed me, actually crawled down a long flight of stairs several times, head first, with great effort and great expenditure of wind and temper, presenting a fine study for an artist; still without producing the desired result. Standing upon their head, riding a hard-trotting horse, blows upon the abdomen, violent electrical shocks, thrusting goose-quills, netting-needles, and other implements, into the uterus, and many other means equally violent, or absurd, are resorted to by the initiated.

Now we say that, as far as we can perceive, there is no violence done to the system by the remedy under consideration. And the only positive effect thus far noticed is a fulness of the generative organs, causing some pain, the same as most patients experience prior to having their usual flow; in one case, where there was acute metritis, the pain was excessive, or materially increased, until the discharge made its appearance; when comfort was restored—proving, as we think, that the action is specific, causing a rush of blood to the generative organs, and thereby favoring a flow, as in the physiological state. In fact, instead of affecting the system violently, it has in some instances, as in the case reported by Dr. Farnham, viz., that of phthisis, produced quite an opposite result, and he was induced therefore to continue the medicine.

1. The first case, that of Mrs. D., was well calculated to test the drug; she was about twenty-six years old; two years since she had a miscarriage of about five months; she recovered slowly; thinks that ever since she has had ulceration of the womb; has had constant discharge of pus, and her strength has gradually failed, until three months since, when she was unable to walk with comfort; had considerable pain in the region of the womb, for which she has been in the habit of taking large doses of morphia daily (viz. from ʒ iv

to 3 vj to 3 vj of Magendie's solution; she has a large deposit of adipose tissue over the whole body; about eleven months since her flow ceased entirely, after being irregular for many months prior; she eats but little; has been in the habit of taking wine or brandy, daily, to a considerable extent; ordinary remedies have failed to reproduce the menstrual flow; I gave her 3 j of the tsa-tsin, to be repeated the following night, if necessary; after the second dose she commenced to menstruate, and continued to do so for three days.

The next month I directed her to take 3 j of the medicine at the time of the expected return of the discharge; she thinking it not necessary, omitted to do so; her flow did not make its appearance; a week after she again took tsa-tsin with prompt effect; she was obliged to take a dose for the several months succeeding, when the discharge became regular.

2. Miss N., of Brooklyn, has gone over seven weeks without seeing anything; in other respects seems well; gave her 3 j tsa-tsin, which was immediately effectual.

3. Mrs. S., Fifty-first street, has uterine ulceration; has borne two children, the youngest of which is two years of age; she is about twenty-six years old, has not seen anything for two months and a half; fears that she is pregnant; gave her two doses of the tsa-tsin without any perceptible effect; she subsequently proved to be in the family way.

4. Mrs. S., of Franklin street. I delivered her of a child nine months since; has not been regular; is about twenty-two years old; has had a discharge occasionally of blood each month; has not seen anything for six weeks, is in other respects well; is rather of a plethoric habit; is anxious to have her monthly discharge; gave her 3 j tsa-tsin, to be repeated, if necessary; no result; have since delivered her of a fine boy.

5. Mrs. H., Madison avenue, has never been pregnant; was married about two years since; I am treating her for uterine ulceration, &c.; she has not seen anything for eight weeks; gave her tsa-tsin without effect; proved to be pregnant, and was safely delivered at full time.

6. Mrs. K., thirty-eight years of age; never before pregnant; fears that she now is so; had uterine disease two years since, for which I treated her; she has been well since; thinks that she has not seen anything for three months; has taken a great many things to bring on her flow. Madame Restell was applied to, and her remedies were causing pain, catharsis, &c., but as regards menstrual secretion, without success; gave her 3 j tsa-tsin; no result; I have subsequently delivered her of a living child.

7. Mrs. P., Bleecker street; is twenty-six years old; has had one child, which is now two years of age; has not had any flow for the last three months; gave her 3 ij tsa-tsin; she has proved to be enceinte; is not yet delivered.

8. Miss H., Nineteenth street, twenty-four years of age; is in

good health, but is irregular; has not had her periods for seven weeks past; gave her ℥j tsa-tsin; the next day her flow came on more profusely than usual; has since been regular, and in larger quantities than before.

9. Mrs. B., thirty-six years old; has had for the last ten years a scant flow, accompanied with great pain, and suffers greatly from plethora, headache, &c.; gave her ℥ tsa-tsin; her flow came on, but not in larger quantity than usual; I subsequently enlarged the cervix with bougies; she has since been greatly relieved, and has continued to flow at regular intervals and in increased quantity.

10. Miss M., Tenth street, nineteen years old; has very scanty discharge; it does not last for more than one day, and very little at that; is well in other respects, except a chronic eruption of acne; has a florid complexion, and a fine physical development; has resorted to the usual means to increase her menstrual flow, without success; gave her ℥j tsa-tsin, which was promptly effectual in bringing on and increasing the discharge; each month, when taken, same result.

11. Miss S., Nineteenth street, has not seen anything in two months; cannot discover any cause for stoppage; gave ℥j tsa-tsin, which was effectual.

12. Miss H., eighteen years of age; has some symptoms of uterine congestion; has some leucorrhœal discharge, with weight in the abdomen; is somewhat plethoric; suffers from pain in the top of her head, and also in the back; is irregular; has not had a flow for six weeks; gave her ℥j tsa-tsin, which was immediately effectual.

13. Mrs. O. has not menstruated during the last three months; has had a child four years since; gave her tsa-tsin ℥j; no effect; she has proved to be pregnant.

14. Mrs. R., Tenth street, about thirty years of age; has had her menstrual flow at irregular intervals, and in very small quantities; the system being relieved monthly, or thereabouts, by a profuse hæmorrhage from the rectum; has been treated for ulceration and congestion of the womb; is very weak and unhappy; eats but little; and although she has used tonics, etc., feels that she is running down, and can exercise but little; a single dose of ℥j immediately brought on the discharge in increased quantity, and by repeating the dose for several months, with the same result; she now declares herself well.

15. Mrs. W., Eighteenth street, a widow; is twenty-six years old, of medium size, somewhat anæmic from hæmorrhoids of several years' standing; has frequent discharges of blood from rectum; has also chronic ulceration of the uterus; came to me, supposing, from the assertion of her physician, who is of the homœopathic persuasion, that she has recto-vaginal fistula; is very much depressed, and feeble; is irregular in her catamenial; has not had a flow for about seven weeks; gave her ℥j tsa-tsin, which was immediately effectual, and she has continued to

the present to be regular; is gaining in flesh and in every other respect.

16. Mrs. S., Fourth street, is thirty-eight years of age; is of a gouty diathesis; has had many severe attacks of the disease, and has considerable deposit of chalky matter; is much reduced in strength and flesh; is very irregular in her menstruation; sometimes goes two or three months; takes but little nourishment, and exercises but little; has gone over three months without seeing anything of her flow; gave her ℥ j tsa-tsin; no effect; repeated the dose without immediate effect, but after a few days the discharge returned, and she has continued to be more regular than previously.

17. Miss V., Houston street, nineteen years of age; has phthisis; has not had her flow during the last five months; has profuse night sweats; is confined to her bed; she has an idea that if her menstrual discharge could be reëstablished, she would be better; gave her ℥ j tsa-tsin, and repeated it several times without any result; she has since died.

18. Mrs. DeW., Thirty-sixth street; is twenty-four years of age; has been married about six months; has metritis, and is annoyed with hæmorrhoids, several of which I removed; has not had her flow for six weeks; gave her ℥ j tsa-tsin; same night her pain in the womb was greatly increased; hysteria was added to her sufferings; applied leeches freely, morphine and belladonna; before morning her flow came on, and she was promptly relieved; has since been regular, and is quite well.

19. Mrs. J., of Haerlem, is forty-six years of age, has ovarian tumor, is very irregular, and has scanty flow, has not seen anything for seven weeks; gave her ℥ j tsa-tsin, to be repeated if necessary: ℥ j caused the discharge, but not in any increased quantity.

20. Miss E. B., Twenty-fourth street, is 15 years of age, is very large for her age, has had her menstrual discharge for two years past, is in pretty good health in other respects, has gone over her time two weeks; gave her ℥ ss. tsa-tsin, to be repeated if necessary; it was several days before the flow made its appearance; has since been regular.

21. Miss V., an actress, is 27 years of age; I was called to attend her for acute metritis. She had just returned from Philadelphia, where she had endured more than usually falls to the lot of a delicate woman, for several months having lived with a man who had not been sober for many weeks past. She had exposed herself to cold, and night watchings for him, until she was completely broken down; her condition somewhat sobered him, and fearing that she would die if she remained longer at Philadelphia, he brought her to New York. I gave her opiates, and applied hot fomentations, together with other treatment, when she gradually improved: she had not seen anything for three months. I also gave her after a few days ℥ j tsa-tsin, which was promptly effectual and hastened the cure. She has since been regular.

22. Miss W., First street, is 38 years of age; has not menstruated for five months, thinks that she is about to have a change of life. She looks to be at least 45 years of age; has suffered from uterine disease for many years. This is one of the worst cases of procidentia in an unmarried woman that it has been my privilege to see; caused by hard work, she being a farmer's daughter, and having done most of the indoor work of the farm. I gave her ʒ j tsa-tsin—and repeated the dose, without effect. After treating her for several weeks she returned home, her womb being supported by an intra-vaginal supporter, improved not only locally, but entirely changed in appearance.

23. Mrs. L., of Johnstown, is 19 years of age, has symptoms of uterine disease, has been irregular for several years, was married three months since, is of fine complexion, well developed, and the picture of health; has not seen anything for five months, she took ʒ j tsa-tsin, which she immediately vomited; she has since repeated the dose without result; has proved to be pregnant.

22. Miss P., Tenth street, is 15 years of age; the secretion came on seven weeks since for the first time; not seeing anything since gave her ʒ j tsa-tsin, with tardy but good result; her flow made its appearance several days after.

23. Miss De F., B. W., is 19 years old, is healthy but irregular; she took ʒ j tsa-tsin with good result.

26. Miss G., eighteen years old, has had epileptic fits since she was twelve, following a severe sickness; she is small in stature and deficient in development in every respect; she was sixteen before menstruating freely, and then not until she had undergone the process of uterine dilatation. She often goes over her time; had done so for two weeks, when I gave her ʒ j tsa-tsin, which was repeated without effect, except a pelvic fulness and some pain; this continued for a few days, when the menstrual flow came on, whether from the use of the medicine or not I cannot tell; several of the cases herein mentioned, acted in the same manner.

REPORT OF DR. UNDERHILL.

Case 1. Miss P., aged twenty years, called upon me in October, 1866, for amenorrhœa of two or three months' standing; face full and sallow, moderately fleshy, furred tongue, constipated, headache, debility, sluggish circulation, pulse 80 and small, eyes sunken, quite too unwell to attend to her duties as school teacher. Advised her to leave school for a time, which she did. Gave her a mild mercurial with rhubarb for a few days, pediluvia with mustard, when, her appetite returning somewhat, gave her an infusion tsa-tsin ʒ j to ℥ viii water, making three doses within three or four hours; seeing no effect, the same was repeated the next day, followed by taking the drug in a little water on the following morning, with no result. She was now placed under pil. ferri comp. for

a week, when tr. guiaci was given, and in the course of a few days was successful in inducing the menstrual discharge.

2. Miss M. applied to me for amenorrhœa, Nov. 3d, of some months' standing, 22 years of age, a fair amount of flesh, countenance pale and sallow, weak pulse, somewhat frequent, furred tongue; anorexia, constipation, headache, occasionally with great inability to take much exercise; some leucorrhœa; had been taking medicine from several physicians without the desired effect; ordered some mild cathartic medicine, containing a small quantity of blue mass, mustard pediluvia at bedtime; at the end of a few days felt better, with an increase of appetite; gave infusion tsa-tsin 1 ℥, divided into three doses, given at a few hours' interval, the dregs the following morning, as in the other case, with no effect upon the return of the menstrual function.

Afterwards got relief by taking pil. ferri comp. with an infusion of tansy.

Dr. Prince.—Dear Sir.—The above two cases are the only ones in which I have used the tsa-tsin in amenorrhœa; having but two cases, this evidence of the negative power of the drug may not be worthy of much consideration, especially in view of your larger and successful experience; but I hand them to you, nevertheless. Yours truly,

February 6, '67. Alfred Underhill.

Dear Doctor.—I regret that the history of the cases in which I have used the tsa-tsin is so very unsatisfactory. Of the five cases the desired result was obtained only in one, so far as my knowledge of the facts would warrant me in stating. Two of the cases did not report, one of them I heard from indirectly as being much better, but no particulars; she went away from the city just after trying the remedy.

One case may be of interest as tending to show that the remedy may be used without fear in delicate cases. A patient suffering from phthisis, and very much reduced indeed (having terrible night-sweats, which compelled three or four changes of linen every night), fancied that if her menstrual function could be restored she should get well. It had been suspended by the disease about five months, and there was a slight effort of nature every month, which I thought might possibly be aided by the tsa-tsin. She tried it and had a remarkably good night, being much less restless than usual, and free from night-sweat, and felt better next day. This she repeated two or three times at the menstrual periods, and each time it seemed to have the same pleasant tonic effect, and lessened if not entirely prevented the night-sweat. It did not restore menstruation, and I did not expect it to, under such circumstances.

The fifth patient, five years married, without children, anæmic, and somewhat emaciated, had ceased to menstruate about three

months; tried the remedy without success, and has since given birth to a fine child.

Sorry that my report is of so little value.

I remain, yours very truly,

H. P. Farnham.

Dr. C. Prince, 157 E. Tenth Street.

New York, February 17, '67.

TSA-TSIN IN SUPPRESSION OF MENSES, BY DR. M. BLUMENTHAL.

I have given tsa-tsin to the following persons:—

1. Mrs. K., married; generally subject to menorrhagia; aborted at seven months; had a good recovery; after four weeks the menses reappeared, but at the next period they missed time; she went six weeks beyond her time. One drachm of tsa-tsin was taken in infusion; on the second day thereafter the menses made their appearance.

2. Mrs. S., married; passed her regular time three weeks; took ʒ j of tsa-tsin in tea, with the desired effect.

3. Mrs. B. always suffered from much pain in abdomen, and bearing down pains at the period of the menses, losing but a very slight amount; took ʒ j of the tea, with the effect of entire relief from pain ever since (3–4 months) and increased flow.

4. Mrs. G. has not had her menses since eighteen months; is thirty-two years old; there is no womb disease; has taken three drachms of tsa-tsin *without effect;* is otherwise in good health. At the periods when the menses should appear, has slight secretion of milk.

5. M. I., aged eighteen, of slight build, and tiny and delicate appearance, small in size, and quite childish in form; has never had her periods, though she suffers periodically from pains in back, &c.; took ʒ i of the tea without effect.

6. Mrs. E., mother of two children, had her courses only once after the birth of her second child; supposed it to be only a temporary suppression; six weeks from time of last show took ʒ i without effect; two weeks later took ʒ i more; no result. Three months and a half have now elapsed, and there is no reasonable doubt of her pregnancy, thus proving the safety of the tea in cases of suspected pregnancy.

Here we have twenty-six cases in my own practice, seventeen of which were successful; five of these had been scanty; of this number, in two, although the flow made its appearance, it was not in increased quantity; the three others of this class were completely successful, and have continued until the present. One of these two cases, after the uterus had been dilated, has continued to flow regularly in increased quantity; the other was a case of ovarian tumor, and forty-eight years of age: it is not to be wondered at that there has been but little increase.

Some of these cases were of long standing, and had resorted to every usual means to effect the object, without success.

There are seven cases that have proved to be pregnant. In these there was no perceptible effect; and we may consequently consider them as successes, increasing our confidence in the remedy, and recommending it to the general practitioner.

We also have two cases of complete failure, one the case of phthisis, and one where the flow had probably ceased altogether, as the lady was of a broken-down constitution from suffering and labor, her womb hanging between her thighs completely prolapsed, and she had not seen anything for many months. She was 38 years old. She had had extensive hæmorrhages from a long polypus, which I removed in this case. Nature may rally, and if so, it is not impossible that the physiological condition may be restored.

I would repeat that in most of these cases, where other diseased conditions were present, we combined such treatment as was calculated to re-establish the normal state, although, in no instance, were emmenagogues, so called, (except tonics,) used.

We will briefly examine the cases reported.

Dr. Underhill mentions two cases where the tsa-tsin was used, and it was not till iron had been given, that the patient was restored to the normal state; how much the tsa-tsin had to do with the favorable result, or what would have been the case provided the remedy had been used as directed, or pushed further, we of course do not know. We must accept them as failures.

Dr. Blumenthal reports six cases, three successful, two without any effect, and one which may be considered successful, as she proved to be pregnant.

Dr. Farnham's report speaks of five cases, but does not give the facts connected with each. Two did not report to him the result; one was dying of phthisis; and one was pregnant; on the whole, so far as we know, giving a favorable account of the action of the remedy.

Dr. Garrish has had sixteen cases; all of which, he informs me, have been successful, except four. I should have been pleased to have presented a full report of these, so as to make our imperfect sketch as full as possible.

And in connection with these nine cases, reported failures, it is impossible to say what would have been the result provided suitable additional treatment had been used; and I would urge the importance of such treatment in connection with the remedy under consideration. I believe tsa-tsin to be one of the most reliable means we have in inducing a natural menstrual flow.

Dr. Harris wished to inquire of the author of the paper, or of the members of the Academy, if any alkaloid or extractive had yet been obtained from this herb, tsa-tsin.

Dr. Prince replied that no experiments had been made, alcoholic or otherwise.

Dr. Chamberlain stated that he had used the tsa-tsin in three cases, in young unmarried women in good health, for the purpose of bringing on menstruation. To one, he gave as high as

four drachms; the administration continuing five days without any effect. There seemed to be no essential properties in the infusion beyond those of common herb tea; it had neither taste nor aroma. In the other two cases, the administration was not carried so far, but the effects were entirely negative. Dr. C.'s observations in these three cases had not induced him to carry the inquiry farther.

Dr. Bulkley wished to know if there was any difference in the samples; to which Dr. Prince replied: the drug he had seen lately seemed not so strong, and had to be pushed farther, which he supposed to be from the fact that the drug had deteriorated.

Dr. Underhill's observations had been limited, the results much the same as Dr. Chamberlain's.

Dr. Harris, in consideration that the herb promised, if the facts which have been reported this evening should be sustained by further experience, to be an important addition to the materia medica, offered the following resolution, which was adopted.

Resolved, That Dr. E. R. Squibb, a member of this Academy, who has greatly contributed to improvements in practical pharmacy, is hereby respectfully requested to submit the herb called "Tsa-Tsin," to such chemical and pharmaceutical examinations as shall ascertain if it possesses any essential alkaloids, or extractive matters, which can be well described, and that he is requested to report his results at his convenience.

YELLOW FEVER.

At the request of the Academy, Dr. Harris reported the following anomalous cases of yellow fever:—

The Dr. said that within the past few days, two persons had died from yellow fever in the Eastern District of Brooklyn; another man had been ill, and was now convalescent; both had well-characterized yellow fever; the liver and the vomit were examined, and were justly considered characteristic, though he regarded no particular anatomical lesion as being absolutely pathognomonic, nor even the black vomit, as he had seen perfect black vomit from pernicious congestive fever; but he believed that these were cases of genuine yellow fever. The three men had been working in a glass-house at Greenpoint, about fifty yards from the mouth of Newtown Creek, where lay a bark—a very filthy vessel from Naples, the filthiest vessel the Dr. ever saw, well calculated to breed disease—discharging her cargo of rotten fruit. The bark left Naples Sept. 17th, and had been in no way exposed in the American Tropics. He said there was a small possibility that a vessel which left Lavacca, Texas, late in May, had come within about 150 feet of the bark and might have been the source of infection, but that was the only probable source known. These men began to fall sick about the 20th of September with rigors,

pain in the back, supra-orbital pain, gastric distress, etc., and the characteristic symptoms of yellow fever.

These were the only cases that occurred in what was supposed to be the infected spot. Hunter's Point, on the opposite side of the creek from Greenpoint, is low and sandy, and the soil of the latter is full of organic matter. About the 10th of Sept., three weeks after the sickness had commenced on board of the bark, the Dr. visited the vessel; he found it empty, except its mud ballast, which was put in at Naples three months before, reaching to within eight or ten feet of where the men slept.

This vessel, the Dr. stated, was manned by eleven Italians, all able-bodied young men except one, who was unaccustomed to the sea; all of whom became ill, two or three at a time, within about three weeks, of the same disease. It was a subject of considerable interest, as having all the appearance of a septic or typhoid fever. There were no remissions, and but little fever, and they began with imperfect rigors and a sense of chillness. He examined very carefully these Italian patients, whom he saw in hospital before and after his visit to the bark. The temperature, by the axillary thermometer, was below the normal standard—down to 95°, 96°, and 97°. These persons passed through a singular course of non-febrile phenomena. There were the coated tongue, sordes about the teeth, bleeding from eroded gums and buccal membrane. The cases were considered anomalous in the three hospitals to which they were sent. One man threw up a quantity of black material, flocculent and acid. Two men out of these eleven died.

The seamen from the Italian bark (the "Chiara") seemed to be suffering from *septæmia.*

The symptoms indicated the fact that some septic poison was producing its usual effects. The blood corpuscles were gradually undergoing destruction by some hidden cause; the pulse and temperature were unnaturally low; and in the last and most marked fatal case, all the serous cavities were lined with pus, and the liver was infiltrated with diffused pus, and contained several small abscesses.

The three cases of genuine yellow fever in the glass-house, occurred after the departure of the "Chiara." They do not seem to have had any necessary or probable relation to that filthy vessel. We have yet to find the source of the infection that produced them.

The Academy then adjourned.

STATED MEETING, NOV. 6, 1867. DR. A. C. POST, PRESIDENT, IN THE CHAIR.

THE following gentlemen were inaugurated Resident Fellows: Dr. Chas. F. Taylor and A. B. Ball.

DR. WM. F. THOMS read a paper upon "Statistical Medicine," and exhibited a number of charts relating to the same subject.

Dr. Bibbins considered those charts relating to lying-in women very valuable, and that some such system, if carried out, would be useful in lying-in hospitals, etc.

Dr. Herzog said:—This interesting and exhaustive paper cannot be considered complete with our present exact knowledge of the influence of atmospheric sediments on, and the reacting influence in producing, zymotic diseases from the soil. The dependence of zymotic diseases upon the soil, has been taken as an axiom by the sanitary science in Europe, and it would be very desirable if the gentlemen connected with this document would make their investigations according to the scientific theories. Of cholera, and typhoid fever, the influence of the soil in direct connection with atmospheric influences is an established fact.

Dr. Harris, by request of the President of the Academy, stated some facts relating to the subject under discussion. He remarked, that, while most cordially appreciating the skill and patience displayed by the author of the paper, it would seem to be very desirable that the Academy should by all practicable means encourage *accuracy* in the elementary facts which go to make up the ordinary tabulated statements in vital statistics.

In the paper just read, we have seen what a great variety of facts enter into every tabulation. These facts and events must be arranged with regard to some important relations they bear to each other. Things similar, or, at least, related to each other, are to be so stated and arranged as to set forth, or lead to, the truth. Now our experience in dealing with the elementary facts of vital statistics shows how very necessary it is to guard against defective and erroneous statements. For example:—Many persons die of congestion of the lungs, and of pneumonia, while suffering from measles. The essential fact is, that measles kills the patient, and that congestion of the lungs is merely the mode of dying in many such cases. Now, if we enter upon the epidemical and meteorological inquiries relating to diseases; or, if we make a public registry of the disease, and cause of death, it is all-important that the facts relating to the same be stated with absolute correctness. That is, we must certify that measles, at a certain stage of progress, terminated in death with congestion of the lungs, etc., etc.

To the educated and conscientious class of practitioners in New York and Brooklyn is the Bureau of Vital Statistics indebted for the truthfulness and fulness that now characterize most of the certified returns received at that Bureau. "And," remarked Dr. Harris, "the fact must be confessed, that, for several months after the organization of the new Sanitary Government, the prospect for needed accuracy in the statement of facts relating to the real causes of deaths, and the accidental complications of those causes, was not encouraging. The public records of mortality for the first six months were full of faultiness in this respect,

and so are all the death-registers of our city for the previous seventy years. But all reputable physicians now seem to strive to make such a statement of the causes of death, and the period of duration of fatal diseases, as will be forever trustworthy and useful."

In regard to the study of the causes of the epidemic and meteorological relations of diseases, etc., we need to guard our minds against hasty deductions and positive final conclusions. In Dr. Thoms's first graphic chart, he ventures to employ the words, "the effects," "producing," etc., when referring to the temperature, humidity, etc. Now, it happened that the terrible mortality in the last weeks of July, 1866, occurred in connection with an unusually dry atmosphere, and that the unusually cool August following had an enormous death-rate. The essential fact was, that other causes of sickness and death were in operation then. And it is safe here to say that there are certain collateral and propagating causes of fatal disease not yet well understood and defined; such, for example, as the condition and physical peculiarities and changes in soils, and the water of the soil, which in the worst of epidemics are probably more direct and important than the temperature and humidity of the air alone. The studies which Profs. Pettenkoffer, Buhl, and Voigt have been pursuing in Europe serve to illustrate this point.

Let us endeavor to find out the true relations which the causes of diseases bear to each other, and to other things in nature; and if every physician truthfully records the events which he observes, he will most effectually aid in establishing the great fund of vital statistics from which our profession, and all mankind, will derive solid conclusions and life-saving benefits. The author of the paper this evening, and all of us, will add to the value of such records of life, diseases, and mortality, just so far as we record truths and study the true relations of facts. We may rest assured that the modes of recording and tabulation will go on improving as knowledge increases.

BLOW-PIPE.

Dr. Post presented on behalf of Dr. Kingsley, of the College of Dentistry of this city, a blow-pipe, which he said will be found by medical men to be very useful. The instrument consists of a flexible tube (to be attached in the ordinary way to a gas-burner), terminating in a metallic pipe; to this another pipe is attached, at an acute angle, through which air is driven from the lungs, producing a flame of much greater heat than the ordinary blow-pipe. The flame can be turned in any direction.

The Academy then adjourned.

STATED MEETING, NOVEMBER 20, 1867. DR. HENRY D. BULKLEY, VICE-PRESIDENT, IN THE CHAIR.

DRS. JOSEPH E. JANVRIN and MICHAEL HOGAN were elected Resident Fellows.

DR. GEORGE BAYLIS was inaugurated.

TALIPES VARUS.

DR. J. N. QUIMBY, of Jersey City, N. J., presented two patients on whom he had used his new method of treatment of talipes varus, by adhesive strips, without the division of the tendons. The first case being that of his own child.

"As a preliminary step, a few days after birth," Dr. Q. said, "I ordered the nurse to wash and rub the child's feet and legs twice a day for two weeks, with a lotion composed of two parts of water to one of alcohol, adding to a pint of this solution ʒ ss of alum. After this hardening process had been continued for two weeks, I then applied the adhesive strips in the following manner: I used three pieces, one being cut so as to fit and cover the whole sole of the foot. This being applied, the second piece is cut in the form of a parallelogram, an inch and a half wide, and long enough to extend from the hollow of the foot to the knee, on the lower end of which there is an expansion of three inches, which forms an angle of 80° or 90°, so as to extend from the plantar surface of the toes to the hollow of the foot, the foot being held in proper position by an assistant. The third piece is cut of sufficient width to extend from the hollow of the foot to the heel, and extending up the leg to the same height as the other. A roller bandage is then applied, commencing at the toes and extending up to the knees, serving to keep the plasters adherent to the limb. During the first two weeks of the treatment the bandages were changed every day and the plasters twice a week; at the same time pretty thorough passive motion was made. During the third week, the bandages were changed every other day and the plasters only once, and in the fourth week the bandages were changed twice a week and the plasters whenever they became slackened; and this manner of treatment was kept up for two or three weeks, when the improvement was so great that I found it was necessary to remove the bandages and plasters only as they became misplaced. The treatment was continued for three and a half months, after which time the little patient wore nothing but a common shoe, the feet being perfectly restored, there being not even the turning in of the toes, which is so common with children who have suffered from this deformity."

Case II. was that of a child now eight months old—the treatment was commenced at four months, but is now discontinued with an almost complete cure. The Doctor said that, perhaps, the best method of treatment at that age might have been division

of the tendon, as he did not propose to cure cases so old as this—his theory being, that he can restore every child commencing at two or three weeks old.

Dr. Q. wished to call the attention of the medical profession to a few important points in reference to this method of treatment.

1st. That the feet can be completely and perfectly restored, however aggravated the deformity, to all their functions, without the division of any of the tendons, when taken at the early period of from three to six weeks; and without even the turning in of the toes.

2d. The short space of time required for the perfect restoration, and that all the cumbersome shoes with steel splints, etc., are dispensed with.

3d. That it causes less suffering to the little patients, and is not so liable to cause irritation, ulceration, or sloughing of the integument, as is sometimes the case with other methods.

4th. That the utility of this treatment is such, that it is not confined to a few expert surgeons; but that every medical man, however remote, in his rural practice may use it with benefit to his patients.

5th. It has a decided advantage over any other method of treatment with children of a strumous cachexia, as it does not in any way retard the development of the foot and limb, and is, therefore, less liable to be followed by weakness of the ankle-joint, and the child can stand upon its feet, if it be old enough, while under treatment, thereby allowing nature to assist in its restoration.

Dr. Garrish said that he was very happy to endorse Dr. Q.'s method of treatment of varus, that he had used it with perfect success in his own practice. He had a child four months old under treatment at the present time, promising a very satisfactory result. The Dr. thought that if the treatment was commenced immediately after the birth of the child, the success would be better, but that there are certain forms of varus to which this method of treatment is not applicable.

He spoke of two mothers. One having six children, four of which had club-feet, and another three children, two with club-feet.

Dr. Quimby stated that his experience in dividing the tendon, if in weak and scrofulous children, was that it is liable to be followed by weak ankles, and that by using the adhesive straps this would have been avoided in some cases in which he had divided the tendon; he would readily endorse Dr. Garrish in commencing the treatment early, if the skin would stand it, and that it was his custom to use alum-water previous to the application of the straps.

ACROMIO-CLAVICULAR DISLOCATION.

Dr. Quimby presented a patient upon whom he was using a new method of treating the Acromio-Clavicular disarticulation

(or, as some surgeons more commonly call it, the dislocation of the clavicle upward, usually over the spine of the scapula) without deformity or impairment of function, in the following manner: Take a piece of old muslin, or linen, folding it about four inches square, and about two or three inches in thickness, and after it has been compressed as firmly as possible, by laying some heavy weight upon it for a few hours, it is then used as a compress over the scapular end of the clavicle. The dislocation is reduced and kept in position by an assistant grasping the elbow, and pressing upward with his right hand, and firmly downward upon the clavicle with the left, if the left shoulder be the injured one, and vice versâ, if it be the right, the assistant standing in front of the patient. Then taking strips of adhesive plaster, cut about three inches wide, and three and a half feet in length, and passing them from the elbow to the shoulder, the forearm being previously flexed at a right angle on the chest, the plaster extending well out on the olecranon process; and after passing three or four of these strips around in the same manner as the first, another piece of adhesive plaster about the same width and length is passed around the outside of the compress and adhesive straps, and so around the neck as to prevent the compress from moving or sliding off the shoulder. Then a pad is placed in the axilla, being secured by tape passing over the opposite shoulder. A roller bandage three inches wide is applied over the whole to keep them more securely in place. This is the only effectual method that I have ever seen that is able to keep these bones in apposition until the shoulder is restored to all its functions without the least deformity or impairment of its usefulness. I have treated two out of six cases that I have had in this way, with the happiest results, and with very little trouble, the dressing not being removed from the time of its application until the cure was complete, a period varying from four to six weeks. The treatment being so simple, and at the same time so effectual and important, I thought it might be worthy of communication to the medical profession.

Dr. Thos. C. Finnell thought it strange that Dr. Quimby should have met with six cases of this disarticulation, they being so rare; he (Dr. F.) during ten years at St. Vincent's Hospital had not seen a single case—but had seen fractures of the clavicle very far out.

Dr. Q. thought his were genuine cases.

The Academy then adjourned.

Stated Meeting, Dec. 4, 1867. Dr. A. C. Post, President, in the Chair.

The President opened the discussion on the "Curative Effects of Blood-letting," with the following remarks; during which time Dr. Bulkley, Vice-President, occupied the Chair:

Mr. Chairman and Fellows:—Blood-letting has been resorted to as a curative agent from a very remote period of antiquity. It was

in general use in Egypt, in Assyria, in Scythia, &c. Podalirius, son of Æsculapius, is said to have introduced it into Greece soon after the siege of Troy. It was extensively used by Hippocrates and his successors; it was used among the nations of modern Europe, and in this country, and until recently has been practised as one of the most important of remedies. Within the last few years a great change has taken place in the use of this remedy, and this change has been carried to so great an extent among the members of the medical profession, that it is now almost entirely ignored in the treatment of any class of diseases. Its entire abandonment has appeared to me to be a retrograde step. I have no doubt that this remedy has been greatly abused. It has been carried in past times to a very great excess, and has been productive of very great evils in its abuses, but I am well satisfied that it is a remedy of real value.

In many cases the abstraction of blood affords immediate relief to the patient's sufferings, shortens the progress of the disease, and brightens the prospect of a cure.

When blood is drawn from a vein by a large orifice, the effect is striking upon the circulation; it diminishes the frequency of the pulse, and the volume of the artery. There are certain cases, however, in which the flow of blood causes an increase in the calibre of the artery, being relieved from the oppression that before existed. I have repeatedly known this to occur in peritoneal inflammation, the pulse becoming full as the blood flows from the vein.

The diseases in which the effects of this valuable remedy are most strongly exhibited, are acute inflammations, and active determination of blood to important organs, most of which are attended with a greater or less degree of local pain; by this pain, however, we are by no means to be governed as to the amount of blood to be taken. In most cases venesection relieves the pain, but it frequently occurs after the abstraction of blood, that other agents are required to relieve the pain. Counter-irritation and anodynes have often a very happy effect after blood-letting. The flow of blood often causes free perspiration. When the flow has been carried to a considerable extent, there is frequently a tendency to syncope, nausea, and vomiting, and sometimes convulsions.

The abstraction of blood as a curative agent has not been confined to any period of life; it has been practised from infancy to advanced age. There is no reason why we should not practise it in children, from whom it should be drawn by leeches. I think I have seen as marked beneficial effects by the application of leeches in infants, as by the abstraction of blood in any form from adults.

As a general rule, in advanced age, general blood-letting should be abstained from, but we have many cases on record of the beneficial effects at advanced age. Guersent bled a man, at the age of eighty-seven years, twice in twenty-four hours. Frank reports a case in which a man eighty years of age was bled nine times during an attack of pneumonia. I am not, however,

disposed to recommend the abstraction of blood at that age; although even at that period of life it seems sometimes to have been useful. At all events, the practice as reported does not seem to have been so destructive to life as is generally believed.

I do not propose to go into any systematic account of blood-letting, but rather to mention a limited series of cases I have myself met with.

The first case was a gentleman of this city about forty years of age. When he came under my care he was engaged in commercial pursuits, in which he had overtaxed his brain; was a man of short stature, short neck, and very plethoric. Was attacked during the extreme heat of summer, with active determination of blood to the brain; there was profound coma with extreme distension of the vessels of the face, which was turgid with blood; there were also violent convulsions. The pulse was full and bounding. I opened a vein in the arm, and bled him copiously. He immediately regained his consciousness, the convulsions ceased, and the cerebral functions were restored to their normal condition. For a number of months afterwards he was in good health. During the next winter he had another attack, and subsequently had several attacks at long intervals during the extremes of heat and cold, and each time was relieved from them by the abstraction of blood. At a later period he suffered occasional attacks of coma, and convulsions, but with much less activity of the circulation. Local blood-letting was then resorted to with benefit. After a considerable period of years he died of apoplexy. It seems clear to my mind that a number of years were added to his life by the active depletion in the early stage of the disease.

The second case was in a young clergyman, whom I was called to see. He had been suddenly attacked with vertigo, while addressing an audience; found him with a partial stupor, with much confusion of mind. I opened a vein in the arm, and the flow of blood was followed by marked and complete relief. The pressure upon the brain was so severe that, if speedy relief had not been afforded, there would have been reason to suppose that the patient would have been permanently incapacitated to perform intellectual labor.

The third case: I was called to see a woman about forty years of age, who was suffering from very severe cephalalgia; she was of small stature and slender; pulse small, but not what we might call a compressible pulse; had been complaining of intense headache. I did not bleed her; but gave cathartic medicines, and applied cold to the head, and revulsions to the extremities. The headache continued. She told me that she had had similar attacks previously, and that, when the physicians who attended her bled her, she was relieved. I was induced to bleed her at her own urgent request, and took about a quart of blood from her; the bleeding gave her prompt relief. I was called during three or four similar attacks, occurring at pretty long intervals,

and on each occasion I resorted to venesection with marked benefit. On one occasion, after the lapse of a number of years, I was sent for to see her, after midnight; found her in heavy stupor—unable to speak. She, however, recognized me, and without being able to speak, pointed to her elbow, indicating that I should bleed her; which I did, and she experienced relief. From that time she has been comparatively free from those attacks.

In another case, I believe, I saved two lives: I was called to see a lady, who had reached about the eighth month of pregnancy—a lady of full habit. She was attacked with vertigo, partial loss of consciousness, pupils were dilated, face flushed, twitching of the facial muscles, and was œdematous; this was before we had a perfect knowledge of uræmia, as it occurs in pregnancy. I had her placed in a half-sitting posture, and bandaged the arm, upon which she fainted. I removed the bandage, and placed her in the recumbent posture. Notwithstanding the occurrence of the syncope, I thought she required to lose blood, but not feeling disposed to take the sole responsibility, I requested a consultation.

Dr. Delafield was sent for, and he agreed with me. I took a full quart, and she bore it well, and was immediately relieved; she went to the full term of pregnancy and was delivered of a son, who has since grown to maturity, and is now himself a father. I think her life was saved by the blood-letting.

Another case: Three or four years ago, I saw a lady who was within a few days of her confinement. She was over thirty years of age, and pregnant for the first time. I found her in a state of anasarca, œdematous all over, but the symptoms of labor had not come on. Her urine was highly albuminous. I began giving her hydragogues and cathartics. Was sent for next day to see her in labor. The pain came regularly, with considerable severity, and at short intervals, but without producing any marked impression on the ostium, which was very rigid.

The patient began to feel pain between her eyebrows, which gradually increased in severity; taking into consideration the uræmic condition of the patient, and the rigidity of the os uteri, I opened a vein in the arm, and bled her freely; and from the time the blood was taken the supra-orbital pain ceased, and the os uteri began to dilate. The labor was completed within a few hours after the blood-letting; the œdema rapidly disappeared, and the albumen from the urine. I felt satisfied that the blood-letting saved her from an attack of convulsions.

I have found blood-letting of some value in certain chronic affections.

I remember a young widow who came under my care, complaining of severe pain in the right hypochondriac region with intense jaundice; and I thought she probably had a biliary calculus or spasm of the biliary ducts. I treated her with anodynes and anæsthetics with benefit, but she had other similar attacks.

The patient went into the hands of Dr. Richard K. Hoffman. He regarded the case as one of inflammation of the duodenum, and bled her from the arm; marked relief was afforded, and the attacks did not return.

A number of years ago, during one of my terms of attendance at the New York Hospital, a patient was left in one of the wards with a severe form of iritis. He had not been bled, but had been put under the influence of calomel and opium; the gums had been touched, and the disease was brought to a stand-still; the aqueous humor appeared turbid, the pupil was irregular, and the sight was very greatly impaired.

I had been in the habit of taking blood at the commencement of iritis, when there were severe circum-orbital pains. Finding a firm pulse, I directed the house surgeon to bleed him, and in two or three days the traces of inflammation were all passed away, and vision was completely restored.

I had under my care at the New York Hospital, a patient with strangulated inguinal hernia; the hernial tumor was of considerable size, and tender to the touch; the usual means had been resorted to for its reduction. I called for a consultation with reference to an operation, but previously directed leeches to be applied to the *tumor*. The leech-bites bled freely, and the hernial protrusion retired within the abdomen. With regard to the advantage attending the abstraction of blood in acute inflammation in young children, I had a case in my own family. My second child, when about two years old, had an acute attack of pneumonia. I applied several leeches over the thorax without much relief. The child was very restless and difficult to manage. I attempted to give a small quantity of tartarized antimony in solution, in a tumbler; she grasped the tumbler and swallowed a quantity which I estimated to be about a quarter of a grain. She became much prostrated, and I gave her a small quantity of brandy and water, and soon after that she fell asleep. When she awoke her breathing was greatly relieved, and there was scarcely a trace of inflammation remaining.

I have had a number of cases of puerperal peritonitis under my care. I have uniformly found good results to follow venesection when resorted to on the first day of the attack, but at a later period the disease is aggravated by the loss of blood. The cases to which I allude were in private practice.

I have had cases of pneumonia occurring in robust persons, and not complicated with tubercles, where a copious venesection in the early stage of the disease has given immediate relief to the urgent pain and dyspnœa, and has been followed by a much more rapid convalescence than usually takes place under other circumstances.

I have been induced to present these cases, with the accompanying remarks, not from a desire to revive the indiscriminate Sangrado practice, which has been so justly discarded, but to

recall the attention of the profession to a remedy which is capable of doing real and capable service in the battling with some of the most formidable diseases to which man is subject.

Dr. Anderson remarked that Dr. Post's observations carried him back to the time when he used the lancet, and with such happy results; and he thought, with Dr. P., that it should be resorted to early. He had a case of a gentleman of full habit, a free liver, who had an attack of congestion of the lungs with breathing much oppressed, taken suddenly and presenting no indication of pneumonia. He had opened a vein in each arm, and the patient shortly recovered. He mentioned the cases of Dr. Cheeseman and others, who had been bled with benefit.

Dr. Joel Foster said that in his early practice, many cases of pneumonia had been cut short by timely bleeding. He thought the remedy of great value in pleurisy; had had a severe attack of that disease himself; was bled about two pounds, and in two hours was bled again, and obtained relief each time. He said that bleeding in pleurisy is now considered almost fatal, but believed that, if he had not been bled, he should have lost his life. When residing in a country village, in 1827, he had seen there a disease similar to typhoid fever, in which, if a small amount of blood was taken early, it was followed by good results; and in cases where it was neglected, it was often followed by congestion of the brain, etc. The Doctor thought that diseases have changed their character, and did not believe that they required such active treatment as formerly. Thought he could remember many cases where life had been saved by bleeding, but that we do not see so many now; that the diseases are of lower grade and less acute. He believed that there are now occasionally met with cases of pneumonia where prompt bleeding would be of benefit.

Dr. Garrish was happy to find that blood-letting is still advocated, and its advantages of the past not altogether forgotten. In reference to disease, he thought it much the same at the present day as in former times, but that there seemed to be a lack of discrimination as to the cases in which the lancet should be used. He said the idea was prevalent, that we are not to use the lancet, but thought that a man ought to be censured just as much for not using it in proper cases, as for neglecting any other part of his professional duty. Dr. Garrish always intends to carry the lancet, but very seldom uses it, because he does not often find cases requiring it, though he had saved many women from convulsions by it; then again was certain he had by it saved persons from death during convulsions. The doctor said, there is no gentleman, who has seen inflammation of the eye, who has not seen the beneficial results of depletion, and he hoped and trusted the time of extremes had passed.

Dr. P. Van Buren said: In his own experience he had witnessed the excellent effects of blood-letting, but the truth was that much prejudice had sprung up in relation to the subject.

Within a few years, the doctor had had several cases of pleuritis, but had refrained, in most cases, from the lancet. In the beginning of his practice it was thought highly important to immediately open a vein; he had seen immediate benefit from it; and the patients got speedily well. He had used the remedy with beneficial effects in puerperal convulsions, and in prolonged labor, where, for instance, the os was unyielding. The doctor thought that if we could dismiss that prejudice, it would be beneficial to ourselves, and that patients could often be more speedily cured by blood-letting than the methods often pursued.

Dr. Sayre said, when he was a student, he saw a child bled to death; since that time he had not tried blood-letting. He did not doubt that there were some cases where the lancet would be of benefit—in a congested condition of the blood-vessels; but thought those cases were extremely rare.

Dr. S. had met with but three cases in fifteen years, where it was considered necessary to use the lancet, though he carries one in his pocket. Local blood-letting, he thought, was well, but general blood-letting was seldom required, and that alternately ligaturing the limbs could be resorted to with very great benefit, in many cases as a substitute for blood-letting; by this means the blood was ready for use again, when required. He did not think this prejudice was derived from the public, but from a higher source; he said physicians did not do it, because they knew better; science had instructed them better; they did not want to go into extremes at all. He was satisfied from his own experience that if the lancet had been used in his own practice, he should not have met with as good success. He believed in the baker and butcher, and in fresh air, rather than in the use of the lancet.

Dr. P. Van Buren said that the subject of blood-letting had been of considerable interest to him for twenty-five years. He had never used the lancet, except in cases of extensive apoplexy; though he carried one. Where the indications for its use were palpable, he could restrain from it no more than he could from pulling a child back from a precipice.

He said physicians do not make the necessary distinction between country and city life; that a strong robust man from the Green Mountains would bear bleeding much better than a man brought up in the enervating influences of a great city.

Dr. Richards believed the reason why Dr. Sayre and others did not use the lancet, was because things had changed. He had seen happy results from the use of the lancet.

Dr. Sayre did not see that so much difference between the former and later times existed, though he confessed that he thought there was a difference between city and country practice.

Dr. Griscom wished the discussion to be continued at a future meeting.

The Academy then adjourned.

Stated Meeting, January 15th, 1868. Dr. A. C. Post, President, in the Chair.

By request, Mr. T. M'Ilroy, of this city, exhibited an operating table. The chief merits claimed for this table are, that by the aid of screws, levers, and hinges, the patient can be placed in any position required. This table had been purchased for St. Luke's Hospital.

Dr. N. S. Davis, of Chicago, was elected Corresponding Fellow of the Academy.

Dr. James L. Farley was elected a Resident Fellow.

ELECTION OF OFFICERS.

The election resulted in the choice of the following gentlemen: For Vice President, Dr. E. R. Peaslee; Recording Secretary, Dr. E. H. Janes; Corresponding Secretary, Dr. Wm. C. Roberts; Trustee (to fill one vacancy), Dr. S. T. Hubbard; Committee on Medical Education (one vacancy), Dr. J. R. Van Kleek; Committee on Medical Ethics (two vacancies), Dr. E. Krackowizer for long term, Dr. T. C. Finnell for short term;—Committee on admissions (one vacancy), Dr. Ellsworth Eliot.

The Academy then adjourned.

Adjourned Meeting, Jan. 29th, 1868. Dr. A. C. Post, President, in the Chair.

Dr. Detmold read the following paper on

DEAF MUTEISM.

Mr. President, I desire to call the attention of this Academy to a subject which, though it has occupied the humanitarian, has hitherto received but little attention from the medical profession. I allude to deaf muteism; and the education of the deaf mute. The subject claims the interest of the profession, not only because it is of great importance in itself, but because it concerns a large class of unfortunate beings who, at first sight, seem to be entirely cut off from all means of education.

The proportion of deaf mutes in European countries is about 1 to 1500, with the exception of those countries where cretinism is endemic. Thus in Switzerland and Baden it is 1 to 500, and in the Canton of Bern, in Switzerland, 1 to 200. In the United States it is about 1 in 2000; and it is observed that the colored race is less subject to this infirmity than the white race.

About the causes of congenital deaf muteism—I say congenital, for if a child becomes deaf during the first year of its life, it will

also become deaf mute—I say of the causes of deaf muteism, we are almost entirely in the dark. We cannot trace it to intermarriage, nor can we assign any other cause; all we know is, that it runs frequently in families, instances where several children in the same family are thus afflicted being of common occurrence, though it is not always hereditary, deaf mute parents often producing children who hear perfectly well.

Pathological anatomy, likewise, throws but little light upon the organic changes, for in some instances the development of the organ of hearing is apparently normal; in other instances, where we find deviations from normal development, it is uncertain whether they are primary, and therefore the cause of the deafness, or whether they are secondary, the consequence of the entire disuse of the organ.

The fact is, we know less about the physiology of the ear than of almost any other part of the body; we do not know the specific functions of the different parts of the internal ear, as we do those of the different parts of the eye. The science of acoustics itself is far behind that of optics. Thus, the theory of the rays of light, their breaking reflection, etc., is pretty well understood—much better than the theory of the sound-waves. Take the simple fact, which I cannot understand, why a sound-wave or vibration of the air, though it may be influenced by wind, that is, it may be impeded or carried on in its course, yet it is not disturbed; that the fiercest hurricane, which will tear the stoutest sails into shreds, will not disturb the delicate vibrations of sound-waves from the smallest tinkling bell; it is only larger sound-waves which can, so to speak, drown smaller ones.

It is needless to mention that in the deaf mute the organs of speech are perfectly developed, and that muteism is simply a consequence of deafness. The children do not learn to speak because they do not hear; the changes which we find in the organs of speech, as the heavy sluggish tongue, the almost atrophied soft palate and larynx, are secondary, the consequence of disuse of the organ.

The education of deaf mutes is a subject of the highest interest. Dr. Johnson considered it as one of the greatest philosophical curiosities, for the deaf mute is apparently cut off from all means of having ideas, and particularly abstract ideas, conveyed to him. It is like building up an edifice without the usual building materials. One of the best and most correct means to measure civilization, is the manner in which society takes care of those unfortunate classes which, by natural defects, are unable to hold their own in the strife and battle of life, as the insane, the blind, the deaf mute, the idiot. In the barbarous ages, these unfortunate beings were not only neglected and trampled under foot in the reckless race of life, but the only duty which an indifferent society felt was to protect itself against danger from them. Thus the insane were kept in irons and chained, and in some countries children, who at

the age of five years could not hear, were put to death; for, there being apparently no means of making them acquainted with the laws, they were considered as dangerous monstrosities. As civilization advanced, society felt not only the duty to protect itself, but also recognized the right in those unfortunate beings to their share of happiness, and even finds itself benefited by enabling them through education to contribute their share to the general good.

Among the men whom history mentions as having interested themselves in the education of deaf-mutes are, in the 16th century: a German, Valentine Ickelsamer, who is said to have published a book on the subject in 1534, which however is not extant now; and a Spanish monk, Pontires, who died in 1584. The first book extant now is by a Spaniard, Juan Pablo Boret, Madrid, 1620. In the 17th century, in England, Wallis; in Holland, Van Helmont and Ammon, who were followed by Sicard, Pereire, L'Epee, and the German Heinecke. The two latter were the champions of the two different systems of education of the deaf-mutes. The Frenchman, the Abbé De L'Epee, advocating the system of dactylology, or talking by signs, which was called the French system; and the German Heinecke, who in 1771 established a public institution in Leipzig, and who taught and advocated already vocalization for the deaf-mutes, which was therefore called the German method.

It is a singular fact, that nearly one-half of all the deaf-mutes are not recognized as such until after their tenth year. This circumstance arises from a variety of causes:

1. The unwillingness of parents to admit even to themselves such an affliction in their offspring.

2. The parents accustom themselves, without being aware of it, to accompany everything they say to the child with expressive gestures and pantomimes which the child understands, and thus they persuade themselves that the child hears.

3. It is exceedingly difficult even for a physician to establish the fact of deafness in a child, and to exclude in the experiment, everything which may lead to deception.

4. Deaf-mute children have frequently a more intelligent look than other children of their age, because they are very attentive in their look, and demonstrative in their movements.

5. Almost all deaf-mute children have, as the parents will tell you, spoken some words. Among a large number of deaf-mute children which have come under my observation, I have found none that have not spoken the words of mamma and papa. This is a curious fact, and, as far as I know, it has never been observed or mentioned by any of the writers on the subject. At first this used to puzzle me, and led me to think that the children had heard and had lost their hearing and speech later, that is, that the defect was not congenital; but I found the same thing so constantly and always the same, that I looked for another explanation,

which is the following, and no doubt the correct one: the children had learnt those words, papa and mamma, which are naturally the first they are taught. I say they had learnt them by sight, that is, by imitating the motion of the lips—both words, mamma and papa, being strictly labial sounds, and therefore easily imitated and learnt. Later, when the child is to learn more words, it forgets again those two which it had learnt at first. In making these observations I have arrived at another curious fact in connection with this circumstance, and that is, that in all languages, ancient as well as modern, civilized as well as uncivilized, the parental relationship is expressed by exclusively labial sounds, which are, of course, easiest learnt by the young child. This brings me to the special point to which I desire to call the attention of the Academy, namely: that deaf-mutes can be taught to articulate. We know that in all deaf-mutes the organs of speech are perfect; they are only mute because they have not learnt to use those organs, for the child learns to speak by imitating the sound it hears. But there is another way of learning to speak. Let me explain it in a homely comparison. You hear a tune, and if you have a good ear for music, you may go to the piano and repeat this tune without knowing the keys which you strike; this is the way in which ordinarily children learn to speak. But if you have never heard the tune, and are taught to strike the proper keys in the proper manner, you will bring out the tune just as well; this is the way in which deaf-mutes are taught to speak. You have there the whole theory of it; the deaf-mute child watches and imitates the motions of the mouth of the speaker and the words will follow like the tune on the piano, only the modulation is wanting, because that can only be acquired by the ear.

This system of teaching the deaf-mute to articulate is now adopted in almost all the Institutions in Europe; on this continent it has hitherto not been generally introduced. Some years ago a commission was sent from this country to Europe, to investigate this system of teaching, and, after careful investigation, that commission reported adversely to the system. Later, Dr. Peet, the President of the New York Deaf and Dumb Asylum, went to Europe, and in his report, confirms the views of the commission. He states that the Institutions in Europe are in no way superior to those in this country, and the attainments of the pupils are generally inferior, and in no case superior to those of our own. In respect to articulation, Dr. Peet confirms the views of Mr. Day, that instruction in articulation is hardly ever of decided benefit, except where the faculty of speech has been acquired through the ear.

It is not my purpose to decide this disputed point, nor do I claim to be competent to do so. In a strictly utilitarian point of view, Mr. Day and Dr. Peet, as instructors in large public institutions, no doubt have good grounds to base their opinion upon; but from a humanitarian point of view, especially if we consult the feelings of

the deaf-mutes themselves as well as their relations, the change from deaf-muteism to mere deafness is an advantage which they no doubt would be unwilling to forego and spare no sacrifice to acquire. It is, no doubt, a progress which no adverse reports can effectually resist.

Last year the Legislature of Massachusetts appointed a joint special committee on the education of deaf-mutes. In the published report of that committee the relative merits of the two systems, namely, of dactylology and vocalization, are discussed at great length, and although I do not think the subject is exhausted, the conclusions of the committee seem to be in favor of vocalization.

There is one point which I beg to state, as being in my opinion of importance, and not sufficiently appreciated in all the discussions on the subject.

It is an established fact that deaf-mutes are more inclined to pulmonary tubercles than other people, the ratio of death from consumption among deaf-mutes being nearly double. This, no doubt, is owing to the want of exercise of the organs of speech, and their want of development, in consequence. If the system of teaching articulation can change the ratio of mortality from consumption, it would be a powerful argument in its favor.

We have lately had a school for teaching articulation to deaf-mutes established in this city. The institution is as yet in its infancy, and is sustained by the liberality of a few private citizens who are interested in the subject, and who, to their praise be it spoken, though themselves confessing the Jewish faith, throw the doors of their institution open alike to the poor Jew and Christian. The principal of the institution, Mr. Engelsmann, is present here at my request with a few of his pupils, and if the Academy will permit, he will show what he has accomplished in a comparatively brief space of time.

There is also at my request an adult deaf-mute present, who was taught vocalization in a German institution, and of course in the German language. He afterwards came to this country, and has acquired, in his intercourse with the people, the English language—certainly a most curious and interesting fact, and a remarkable illustration of the wonderful resources of the human mind under apparently insurmountable difficulties. This gentleman is now, in partnership with another deaf-mute, carrying on an extensive and remunerating business as lithographer, going about in our business community, receiving orally his orders for work.

I beg to make one more remark, which I consider of some importance. All deaf-mutes are not entirely deaf, but where the sense of hearing is so faint that the child does not hear ordinary conversation, it will be practically deaf-mute. I have for years adopted the plan, where I can detect traces of hearing, to let the children use ear-trumpets, for the purpose of trying whether by such artificial means the hearing can be sufficiently

increased to enable them to learn speaking. In one or two cases the experiment has been entirely successful.

Mr. Englesmann, a teacher of deaf mutes, was introduced with his pupils, for the purpose of illustrating the success attending the method of teaching articulation. The pupils were asked several questions by the President, Dr. Detmold, and their teacher, and answered readily by articulation and writing on the black-board. A deaf-mute gentleman, a German by birth, was also introduced, and questioned both in English and German, and gave intelligent answers by articulation in either language.

Professor Peet, of the New York Deaf and Dumb Asylum, was introduced, and spoke at some length in defence of the sign method of instruction, as the true philosophical method of educating deaf mutes. He stated that the pupils of the New York Asylum, on Sundays, receive religious instruction by the sign language, which he regards as the most practicable, as any one can learn the alphabet in half an hour.

Dr. Detmold said that he understood from the census, that about one-half the deaf mutes are able to support themselves, and he wished to know from Professor Peet if any calculation had been made in this respect.

Professor Peet replied, that the records of the New York Asylum showed that more than nine-tenths of its pupils were self-supporting. He could speak only from these records.

Dr. Blumenthal spoke at some length in support of the system of teaching articulation, and gave a history of the efforts made to introduce into this country the system of educating deaf mutes.

Dr. E. Eliot advocated the sign system, and read an article on the subject from the *New Englander*.

The Academy then adjourned.

Stated Meeting, Feb. 5, 1868. Dr. A. C. Post, President, in the Chair.

Dr. D. B. St. John Roosa read the following paper on

Etiology of Congenital Deaf Muteism.

Mr. President and Fellows of the Academy:—The brief remarks which I design to offer upon the general subject which has been already entered upon by one of our distinguished Fellows, will bear solely upon the Etiology of Deaf Muteism—and since the acquired form is comparatively well understood and appreciated, I shall speak chiefly of the cases usually classified as congenital.

We may assume, in the beginning, that the muteness which helps to give a name to the condition is dependent upon the deafness.

The deaf child who never hears a voice becomes dumb, simply because he is ignorant of the nature of articulate speech, and does not know how to use his vocal organs. Hence, the laryngoscopic examinations which have been so largely made in deaf muteism seem purposeless, unless the object be to learn whether non-use of the vocal cords has produced atrophy of the structure, or similar changes. There can be no question but that the deaf mute child, in the ordinary meaning of the term, is born with the vocal organs in a normal state of development. There are, it is true, a few cases of dumbness unaccompanied by deafness. Sir William Wilde, in his vital statistics of the Irish census, gives 723 as the number of such cases, out of a whole number of 12,195 of so-called deaf mutes. We may disregard these, however, as not belonging to the category which we are about to consider—and speak only of those cases where the dumbness is the simple consequence of the inability to hear, and hence to speak.

We have then essentially to inquire, not into the causes of the dumbness, but of the deafness.

We may divide deaf mutes into two great classes. 1. Those in which the deafness has occurred after birth, from some traceable cause, which is generally an inflammation of a part of the auditory apparatus. These are the so-called *acquired* cases. They form a very large proportion of the whole number, as is well known. In Ireland, quoting again from Wilde's statistics, more than one-half plainly belonged to the acquired; and other statistics exhibit the same proportion.

The second class comprises those who are assumed to have been born deaf; the congenital cases. As we have said before, it is of the etiology of these that this paper is intended to speak.

It would be interesting to note, did time and the occasion permit, the various causes which have produced these acquired cases, which do so much to fill our Deaf and Dumb Asylums. We should find that very many of them were preventible, in the ordinary sense of the word, and that medical science has somewhat to reproach itself with, in the occurrence of so many of such cases. We should also see that a great many of them require the advice and assistance of a physician able and willing to examine and treat aural diseases, in order to check ulcerative processes in the essential parts of the organ of hearing, to relieve patients from the annoyance and discomfort of hypertrophied tonsils, granular pharyngitis, foreign bodies in the ear, etc. But we pass on. The causes of these acquired cases are sufficiently plain. They have never been drawn into that vague etiological field which is subdivided into such indefinite parts as "unequal ages of the parents, intermarriage of blood relations, direct hereditary transmission, causes operating during gestation, through the excited imagination or nervous sensibility of the mother, etc." It is universally conceded that they proceed from the same causes as those which may produce inflammation of the auditory apparatus in any person,

e. g., the sore throat of scarlatina and measles, meningitis, exposure to cold, &c., &c. It is an easy task to trace out the remote causes from these proximate ones in acquired deaf muteism.

I may pause, however, one moment more, before entering upon the subject of the etiology of congenital deaf muteism, to state that, in an examination of the inmates of one of the deaf and dumb institutions of our country, with my friend Dr. Beard, we found one boy who could hear distinct conversation addressed to him alone, the speaker being near at hand, who was, much against his will, being educated as a deaf mute. We were informed that his parents stated that they could neither get a teacher to take the necessary trouble to educate him through the ordinary medium, or a physician who thought it worth while to treat the profuse suppuration existing in the ear. Such a state of affairs hardly requires comment.

The causes of congenital deaf muteism have been usually divided into the remote and proximate—the remote have been quite often and fully written upon, while the proximate have been generally ignored; although superintendents of Deaf and Dumb institutions have given quite elaborate tables of supposed causes. If we admit every one of them to be valid, they are of little value in a scientific point of view, but chiefly interesting as curiosities. We then only know that certain classes of persons, who are living under the influence of certain conditions, are apt to have deaf mute children. It leads to no knowledge as to the condition of the auditory apparatus, the changes which have taken place there—in other words, we have no idea of the proximate causes. But these tables are not reliable, as well as being nearly useless if they were so. They are to a great extent purely a matter of guess-work. Those who adduce them admit their imperfection. Many of the purported facts contained in them are obtained from parents with very little intelligence, or they have been surmised by a non-medical observer, who believes that a severe fright to the mother, the marriage of cousins, etc., will produce deaf muteism. Besides, numbers of the children are foundlings who have no history at all, but whose cases are classified as congenital.

Such tables lead us to no result, beyond an interesting consideration of the remote causes which may induce deaf muteism, dependent upon proximate lesions of which we know nothing. An investigation of the proximate causes must be made, and this can only be done objectively, that is, by the examination of the individual parts of the auditory apparatus in the individual cases. In such an examination of the causes of congenital deaf muteism, we may subtract a certain number, and always, so far as my observation extends, a small proportion, of cases in which the general appearance and habit of action, facial expression, intellectual activity, show with a reasonable amount of certainty that the want of hearing is due to an arrested or imperfect

development of the auditory apparatus, and that the deafness is only a part of an anomalous state of the whole organism.

These cases are unmistakable; and an examination of the membrana tympani and the pharynx always shows that the changes there are not enough to account for the deafness. Dissections in such cases have demonstrated that the semicircular canals of the labyrinth have been often wanting. Atrophy of the auditory nerve has also been found. These cases are interesting in the same way that idiocy is interesting, but they cannot be said to afford an opportunity for a practical study of morbid appearances in the organ of hearing.

One district of our country, where the inhabitants are very much isolated from the world around them, and close intermarriages are very common, furnishes almost every year quite a contingent of such forms of deaf muteism to one of our institutions.

We may consider the etiology of this class as identical with that of idiocy, feeble intellect, or partial development of other parts of the body, spina bifidis, coloboma iridis, etc. We should not consider them in the same category with those cases of deaf muteism where the intellectual activity is normal, and the whole organism with the one exception of the organ of hearing is in working order. Yet in the statistics and remarks which I have been able to find on the causes of the so-called congenital cases, all of them that are not positively known to be acquired have been classed together; and we are left to infer that deaf muteism pure and simple, existing in a child with vigorous intellect, uncomplicated by other anomaly, may result from the same paternal influences which, experience teaches us, sometimes produce that form which is united with semi-idiocy.

This is an idea which seems to me incorrect. Not enough distinction has been made between these semi-idiotic cases where the cause is sufficiently plain, and the remainder, where no external examination will give any clue to the objective condition of the auditory apparatus. Reasons have been given, however, for the want of knowledge on this latter point. It is stated in one report that, "in most cases of congenital deafness we are left wholly in the dark, even as to the condition of the organs of hearing. They are too deeply situated for more than a superficial examination in life, and opportunities for dissection of subjects known to have been deaf mutes rarely occur."

In view of the absence of any attempt to use the means at our disposal for the examination of the ear, in the cases of deaf mutes, and the fact that no physician competent by previous study and experience to examine or treat the ear, is, so far as I know, connected with any deaf and dumb institution in this country, this statement can scarcely be allowed to pass unchallenged. It is not correct. Our means for examining the auditory apparatus are quite as good as those of any internal organ of the body except the eye, if that be considered an internal organ. They are

much better than those at our command for inspecting many of them. We may examine the membrana tympani, the pharynx, the mouths of the Eustachian tubes, and, in a tactile manner, we may investigate the condition of the Eustachian tube by means of probes, catheters, and bougies.

Electricity, and certain physiological tests now in course of trial, promise to still further facilitate our investigations, by giving more definite conclusions than those from exclusion, as to the condition of the auditory nerve. Besides, the histories of the cases, which in many of the cases—more than 300—which have come under my personal observation, are often very decided and clear—such as that, the child having once heard well, and noticed sounds, had a severe inflammation of the head, generally called brain or spotted fever, got well, but was found to be deaf. These histories, taken in connection with the objective appearances, often allow us to come to very definite and tolerably certain conclusions as to the cause of loss of hearing power. It has been the habit of medical writers on deaf muteism to discard these histories almost altogether, and to conclude that the children to whom they are attached could never hear. The experience of Troltsch, Voltolini, and, I may be permitted to say, of myself, does not seem to allow us so readily to dismiss them. It is certainly very probable that young children are peculiarly liable to inflammation of the ear. The membranous labyrinth of children is peculiarly disposed to inflammation, and, as Voltolini (*Monatsschrift für Ohrenheilkunde*, *No.* 1) has shown, it is apt to be mistaken for simple or cerebro-spinal meningitis. In inflammation of the labyrinth, however, there are usually intermissions in the pain, the attacks come on very suddenly, at any season of the year, and, as long as consciousness remains, the patients seize hold of the head. When they recover, it is with absolute loss of hearing and a staggering gait. Physiological experiments have shown that this staggering gait is a constant symptom of injury to the semicircular canals.

Voltolini states that the delirium of meningitis is always constant, and that an intermittent delirium depends upon sympathetic irritation of the brain.

I believe that I have seen a number of cases of deaf muteism resulting from inflammation of the labyrinth. I recall one where the history is identical with those of Voltolini, seen last summer for Dr. Hackley; and when the patient had recovered with the staggering gait, which Voltolini assumes to be pathognomonic. It may be still further remarked that there is not apt to be any paralysis in these inflammations of the labyrinth. When we remember how many cases of secondary meningitis, resulting from suppuration in the ear, have been thought to be primary, until a post-mortem examination has shown what an aural one would have previously demonstrated, we may believe that Voltolini is correct, and that we have here another proximate cause for

some of the cases of deaf muteism, and, I believe, of some of those which are classed as congenital.

Troltsch has shown that in the ears of half-starved foundlings, which he examined in many instances, purulent processes were found to exist in the cavity of the tympanum; and here may be traced another cause for congenital deaf muteism. I suppose that mal-nutrition of parents and offspring may be traced as the remote cause in these latter cases, and that causes such as intermarriages of blood relations, *et id omne genus*, had very little to do with them. I suppose that if the best blood of our continent and race should be mingled with the noblest of the old World, and that if the parents should by some train of circumstances be for a long time half fed, that their offspring, even if as widely separated in consanguinity as an Emperor of Russia and a President of the United States, would be liable to morbid processes in the ear which would produce deaf muteism.

In conjunction with Dr. G. M. Beard, I attempted during the past year, in 296 cases, which have already been published in the *American Journal of Medical Sciences*, Vol. LIII. p. 399, an examination of deaf mutes which should be entirely objective, and tend towards ascertaining the proximate causes of their deafness. It was supposed that, if enough morbid appearances could be detected in adults, who had lost some or all of their hearing power late in life, they could also be found in children; nor were we disappointed.

In 296 cases, 182, or 61 per cent., were classified as congenital, and in only 30 cases of this 182, were there insufficient changes in the membrana tympani and fauces to account for loss of hearing power. So far as such a limited number of cases teach anything, they show that congenital deaf muteism, or deaf muteism occurring at such an early period of infantile life as to be classified as such, results, except in those cases where there has been arrested, or absence of, development, from identically the same causes as deafness occurring at any period of life. These causes are—

1. Inflammation of the middle ear, resulting in adhesions, anchylosis of the ossicula auditus, thickening and opacity of the drum, swelling and imperfect action of the muscular portion of the Eustachian tube, &c.

2. Inflammation of the labyrinth, resulting in thickening of the membranous labyrinth, deposits in it, destruction of the semi-circular canals, &c.

By adding to these two, the one so often alluded to in the course of these remarks—absence of some part of, or arrested development in, the organ of hearing—we have three proximate causes which are at once scientific and plausible. Upon this structure we may build an edifice of deduction, which will be at once solid and beautiful. From them the practical physician may adduce prophylactic laws, which may aid him in diminishing

the number of the truly unfortunate class—deaf persons who have become mutes.

It will thus be seen, I hope, from the foregoing remarks, that it is believed by your speaker that all attempts to trace out the causes of congenital, as well as acquired, deaf muteism, must be made in an objective manner. As has been indicated, we have ample facilities for this on the living subject and the cadaver. After such investigations, we shall not find the subject wrapped about in fearful mystery, but as intelligible as any other field of medical investigation.

But, on the other hand, let us scrutinize with minute and critical eye every subjective classification, especially when made by those unable to judge of the normal appearance of an organ whose functions are lost, and let us give little credence to conclusions drawn from tables, however elaborate, that deal only with incomplete histories and remote causes, to the neglect of the only safe method of investigation, that which begins at the centre and works outwards. Thus it will be seen, Mr. President and gentlemen, that these remarks are only a plea for the examination and study of the organ of hearing in all cases of deafness, in order thus to learn their etiology.

Dr. Detmold stated that he wished to call attention to the facility with which children who have once heard, acquire the ability to articulate. Children who have lost their hearing before learning to talk, learn vocalization much more easily than those whose deafness is congenital. He does not attribute this difference to the fact that one child has heard in early infancy, and another has never heard; but rather to some relation of the facial to the auditory nerve. He hopes the subject will be investigated.

THE THERAPEUTIC EFFECTS OF BLOOD-LETTING.

Dr. O'Sullivan said:—Mr. President. The discussion on blood-letting, and the information elicited on that subject at a former meeting of the Academy, has been to me, one of the Junior Fellows, a most interesting and instructive one. The earliest lessons which I have been taught in the study of medicine have been intimately connected with the ideas prevailing at the time as regards blood-letting, and which governed the practice some years since.

When I commenced practice, I determined with all the zeal of a young practitioner, to test the merits of blood-letting thoroughly. In the neighborhood where I have resided for the past eleven years there are several large foundries and machine shops, where hundreds of stalwart men are daily employed; here was a wide field for investigation, and so enthusiastic was I that for several years I never went without a lancet in my pocket, and with a full determination to keep it well employed. I however very

soon began to perceive that among the class of patients referred to, and who were apparently well able to stand depletion, I had to alter my views very materially, and finally to abandon almost entirely the use of the lancet, especially in diseases affecting the respiratory organs. Now, sir, whether this is owing to the fact that this class of patients reside in ill-ventilated and crowded apartments, or that in their daily avocations they are constantly breathing a vitiated and poisoned atmosphere, or else to climatic reasons, I am not at present prepared to say. The fact however remains, that a certain degree of enervation exists among this class of patients, and that we must be very careful how we deplete them. In acute diseases of children, I have never pursued the practice. I can say with a clear conscience that I have never applied a single leech, but on the contrary sustained my little patients from the beginning in every possible way; locally, the oil-silk jacket and poultices sufficiently fulfil the indications for depletion. In this connection I will briefly mention the history of an interesting case which occurred in my practice some eight years ago, and which I think strongly sustains my views on the subject. In January, 1860, I received a telegraphic despatch urgently calling me to Baltimore, to see a relative who was a student at the Theological Seminary. I left immediately, taking the night express train, and arrived at 5 A.M. I proceeded at once to the seminary, and found the patient in a very debilitated condition, he being in the second stage of pneumonia. I was informed by the nurse, and subsequently by the medical attendant, that he had been without any sleep for three days and nights, and it was evident to me at a single glance that if sleep was not speedily induced, delirium must ensue, and with necessarily an immediate fatal result. I was further informed by both physician and nurse, that at the onset of the disease he had been bled very freely (a large basinful of blood being taken from him), and that considerable debility had ensued, and from which he had, up to the time I saw him, very imperfectly rallied. A condition of affairs which can readily be imagined when I mention that he was a mere anæmic youth of nineteen years. Of course, I need not say that the antiphlogistic regimen was also pursued. On auscultation I found he had a very extensive pneumonia involving both the lower and middle lobes of the lung. There had been applied extensive counter-irritation, on the obsolete theory of its furthering resolution, a mode of treatment which the best clinical observers of the day have so fully refuted. In relating particulars, the attending physician expressed his surprise that, notwithstanding the fact that the patient had been taking large doses of opium in concentrated form, no sleep had been procured, and that in his opinion the case was very serious, and an immediate fatal result probable. He asked my views of the case, and what I would suggest in the present emergency. I stated to him that I very much regretted the treatment he had pursued in the

earlier stage of the disease; that in so young and weak a patient, instead of depletion, sustaining treatment of a decided character should have been pursued from the very beginning; that had such a course been pursued, the solidification would not be so extensive, nor the then existing extreme debility. I suggested at once the most heroic treatment, sustaining the patient with beef tea, tonics, stimulants, etc. Being then unacquainted with any of the profession in Baltimore, I telegraphed to a distinguished Fellow of this Academy, who kindly sent me the names of several prominent physicians in Baltimore, with one of whom we had an immediate consultation, and who fully endorsed my views of the treatment of the case; and on the first night, on a few grains of Dover's powders, he had three hours of most refreshing sleep, resting as placidly as an infant. I myself with the nurse watched the patient the whole night. Now as to the sequel. The pneumonia was cured. But the patient was lost, as he died of phthisis some six months after. The deduction I have made as to the immediate and predisposing cause of the fatal result in this case, is attributable to excessive depletion on the one hand, and on the other to defective nutrition. I would say further, that the young man inherited no predisposition to the disease of which he died, and his relations on both sides were not tainted with the disease, many of them living to an extreme old age. During the whole collegiate course of this patient in this city he was under my supervision, and was never to my knowledge sick for one day.

The Academy then adjourned.

STATED MEETING, FEBRUARY 19, 1868. DR. A. C. POST, PRESIDENT, IN THE CHAIR.

DR. CHARLES HORACE OSBORNE was elected Resident Fellow.

DR. JOHN H. GRISCOM read a paper "On the Therapeutic Value of certain articles of the Materia Medica of recent Introduction, treating principally of the Sulphite Salts of Soda, and Glycerine."

DR. PEASLEE said, the Sulphite of Soda has received but little attention in this country till within the last two years. It was first recommended highly by Prof. Polli, of Padua. He (Dr. Peaslee) had used it in cases of septicæmia, and it seemed to him decidedly to diminish it, if it did not even arrest it. He generally gave 10 grains every 4 hours, together with sulph. quinine and stimulants. He had also prescribed it in cases of erysipelas of the hand and face, with good results, in the dose just mentioned; at the same time constantly applying to the part affected a solution of it, of ½ oz. to 1 pint of water. He had found in some instances that the disease did not spread at all after its application. It is also a valuable remedy in the low forms of scarlatina; and for fetid eructations attending some cases of dyspepsia. It ar-

rests fermentation; and this is usually the cause of the eructation. Dr. Peaslee had also used the hyposulphite and the bi-sulphite of soda; but prefers the sulphite. Theoretically, the hyposulphite should be given in twice as large, and the bi-sulphite in half as large doses as the sulphite—the sulphurous acid being the active element in each salt. But the bisulphite has a more unpleasant taste and a very unpleasant odor.

Dr. Van Kleek has used the sulphite of soda in zymotic diseases; he sometimes prescribed the hyposulphite and sometimes the sulphite. He remarked that many of the apothecaries did not know any difference between them, they both having the same taste.

Dr. Farnham said that several druggists had told him that there was no difference between the *hyposulphite*, the *sulphite*, and the *sulphate of soda;* that one was sometimes put up for the other.

Dr. Griscom also had known the *sulphate* to be put up instead of the sulphite.

Dr. Bulkley said: With regard to the hyposulphite of soda, he used it several years ago, and thought there was no difficulty in getting the salt at that time, as it was used in the arts; though he found that it was a very uncertain salt, and very apt to gripe the bowels. With regard to the *sulphite* of soda, Dr. B. had not much practical knowlege.

Dr. Bibbins said: It had been stated that there was a good deal of difficulty in obtaining the article in question, but Dr. Griscom stated that it was kept by almost all good apothecaries.

Dr. Peaslee stated that it used to be difficult to obtain it, as the apothecaries did not at first know the distinction between the three salts; but that it is now generally understood.

Dr. Peaslee introduced some specimens of vaginal suppositories, of three different colors, to indicate their composition.

Dr. Bibbins said: Glycerine, when applied to the mucous membrane, for instance chapped lips, sometimes produced a good deal of irritation, but that, he thought, was due to the impurity of the article.

DRY CUPS.

Dr. Bibbins called attention to the beneficial effects obtained by increasing the size of dry cups. He had seen excellent results from the application of large-sized cups (size of ordinary goblets) in acute lumbago, rheumatism, and pleurodynia, relating the particulars of an interesting case.

The Academy then adjourned.

STATED MEETING, MARCH 18, 1868. DR. A. C. POST, PRESIDENT, IN THE CHAIR.

DRS. MICHAEL HOGAN, CHARLES HORACE OSBORNE, and JOSEPH E. JANVRIN were inaugurated Resident Fellows.

DR. E. R. PEASLEE exhibited a fibro-cystic extra-uterine tumor which he had removed on February 22, 1868. He said that he first saw the patient about a year ago, when she had symptoms of ovarian tumor; saw her again in July last; she then went on to February 22d, when she strongly urged the removal of the tumor. It was found to be attached by very many adhesions, the adhesions being like columnæ carneæ, different from anything he had ever seen before, and every one of these adhesions bled. At least twenty of them were tied, and it took three hours to stop the bleeding. The pulse was controlled by the use of veratrum viride, and at present there are no unpleasant symptoms.

DR. CHARLES. F. TAYLOR read the following paper:

THE PATHOLOGY OF LATERAL CURVATURE OF THE SPINE.

MR. PRESIDENT:—The lateral curvature of the spine is the most frequent deformity to be met with among the higher classes of civilized society. It is not an organic or fatal disease, but it produces a very unsightly deformity, injurious to the patient's future prospects in life, and often lowers the tone of the general health much below the normal standard.

Those who have written on the lateral curvature have suggested various theories as to its cause or causes. Some of these theories are plausible, and many far-fetched; but without stopping to enumerate these, or to discuss the subject of the impossibility of accounting for this deformity upon a single hypothesis, my purpose is to present briefly my own convictions of the pathology of this affection. I need hardly say that these views are the result of much experience. What I shall say will be but the established facts of my experience.

1. In the first place, a lateral curvature may be induced by many causes. There are not less than six or eight different classes of lateral distortion of the spine; and although some of these are relatively infrequent, they are all liable to occur, and no one can properly pretend to a knowledge of these cases who is not familiar with every form which may exist. The lateral curvature is not an irregular curving of the spine; an indefinite sigmoid or simple curve, which some who have paid but slight attention to the subject seem to imagine. But it is very positive and uniform in its characteristics, varying only in types as causes vary, and depending in most cases on causes extraneous to the spinal

column itself. In fact, there is not one case in a hundred where a lateral curvature is caused by any disease of the spine itself. On the contrary, the spinal column is acted on and distorted by distant causes. Hence we need not look for pathological symptoms in the spine itself, or for symptoms relating to the spine. For the spinal column is distorted, not diseased, and to attribute to the spine the symptoms which may happen to occur in conjunction with a lateral curvature is a serious confusion of symptoms. If physiological disturbance should happen to exist at the same time with a lateral curvature, it would depend on other causes, and the so-called spinal symptoms and the curvature have only an accidental relation to each other.

I will first call your attention to that form of lateral distortion which is caused by a difference in the lengths of the lower extremities. I do not allude, in this connection, to the distortion of the spine caused by serious impairment of one lower extremity, or lameness; in such a case the cause would be sufficiently apparent. But there is a form of lateral distortion, easily detected by its peculiar aspect, depending on a considerable normal difference in the lengths of the two lower extremities. When one leg is

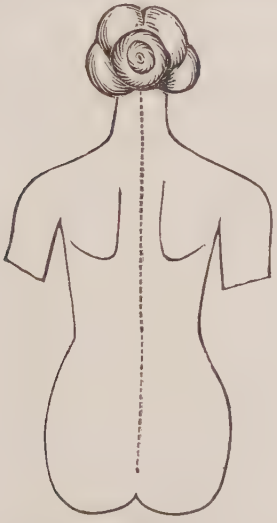


Fig. 1.

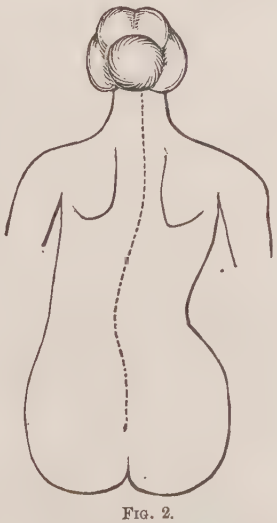


Fig. 2.

considerably longer than the other, one side of the pelvis must be lifted higher than the other, with the effect of throwing the spinal column out of its vertical position in a lateral direction. In this case the principal deformity is in the lumbar region, the chest being but slightly affected. I have endeavored to give you an idea of this form of lateral curvature by the drawing, Fig. 2; Fig. 1 representing the natural form.

This is by no means a rare deformity. Among the abnormalities of the body, a difference of the lengths of the arms and legs from unequal developments is frequently met with by any one accustomed to making careful personal examinations. Of course no treatment directed to the spine itself can have the slightest effect on this distortion.

2. Another deformity—in this case affecting the shoulders principally, and the spinal column but slightly—arises from quite a different source. If you will refer to figure 3, you will observe a remarkable unevenness of the shoulders. Now this deformity of the shoulders is not due to a distortion of the spine, as might be supposed, and is really true in other cases; but it is owing to unequal tonic strength of the trapezii muscles, which allows one scapula to slide down below the other. There is at the same time a want of muscular fulness in the belly of the muscle; which, when contrasted with the other side, makes an apparent curve of the spine in the upper dorsal and cervical regions. The lower angle of the displaced scapula also presents below the level of the other, and altogether makes the deformity very considerable, and all the worse because it is wholly in sight. Of course a tonic spasm of the trapezius muscle on one side would have the same general effect of displacing the scapulæ, and making them uneven, that relaxation or feebleness of the other would do. Unequal tonic action of correlated muscles is not unfrequent, and in certain situations, as in the present instance, a deformity must follow. And here again no treatment directed only to the spinal column could have the remotest influence on the deformity. It is evident that the same influence may be acting in other locations than the muscles which elevate the shoulder-blades; but I present that example which best illustrates the class to which it belongs.

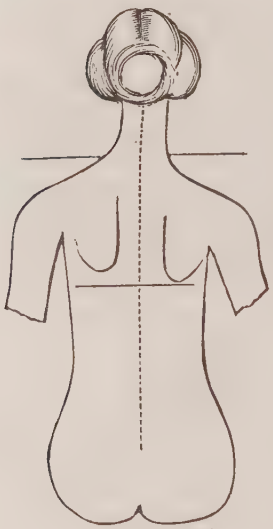


Fig. 3.

3. The next class of spinal distortions to which I invite your attention has its origin within the pulmonary cavity. There has been much controversy and many speculations as to what constitutes the difference, and what causes the difference, between the single and the double or triple forms of lateral curvatures. Such speculations could only be indulged in by persons not sufficiently familiar with the appearance and history of these cases. There is really nothing to excite comparison with each other. I shall expect to show, by and by, that the common legitimate curvature always

is, and always must be, multiple; that is, it must be composed of two or more curves. All the rest are *irregular distortions* of the spine, as the column is forced in one direction or another by the action of special causes. I never yet saw a single curve which could not be traced to some special cause; while I shall show further on, that only one class of cases, and that the double or triple curve, depends on a *general* condition of the system.

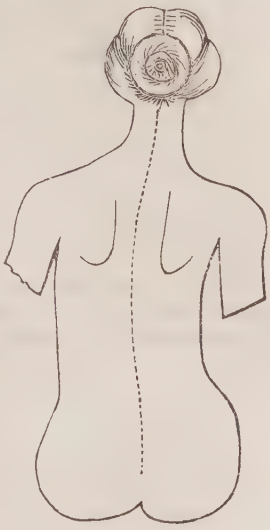
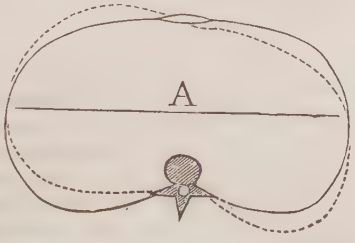


Fig. 4.

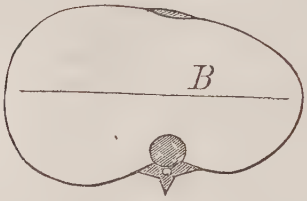
The most common cause of the single curve—such as is represented in figure 4—is found within the chest. Unequal play of the lungs begets difference in the muscular power and the size of the sides of the chest. If this begins at an early day, and continues for a long time, unequal development of the upper portion of the trunk takes place, and distortion of the spinal column must follow.

The history of a large proportion of these cases can be traced to long-continued partial occlusion of one lung, from which the patient ultimately recovered, so far as regards the pulmonary disease, but with a certain amount of physical damage remaining, as shown in the unequal capacity of the two sides of the chest, with the necessary spinal distortion. In this class of cases there is always such a marked difference in the form of the chest, when compared with the legitimate lateral curvature, that I do not see how it has hitherto failed to attract attention. In the legitimate curvature, the long diameter of the chest is changed from the lateral to the diagonal position; while in the class now under consideration the long diameter of the chest is not changed, but one side of the chest remains partially undeveloped, the chest on one side appearing compressed both anteriorly and posteriorly. This will be seen by reference to Figures A and B, where A represents a transverse section of the chest in legitimate curvature, and B a section in distortion from unequal development of the two sides of the chest.



4. There is one class of cases —that which I shall next consider—in which the distortion has its origin in the osseous structures themselves. A defective nutrition of the bony frame-

work of a rachitic nature produces a very characteristic deformity. This I have endeavored to show in Fig. 5 of the drawings. The deformity is often very great. It generally begins to develop itself at an early age. This deformity has nothing to confound it with any other deformity of the frame—even if the history of the case were not sufficeint to indicate its pathology.



5. This must not be confou d with the distortion caused by muscular spasm on one side of the body, as shown in Fig. 6. This was the case of a young girl who was an unfortunate victim of masturbation. She was drawn down much more than I have

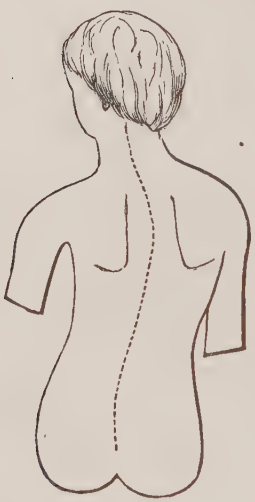


Fig. 5.

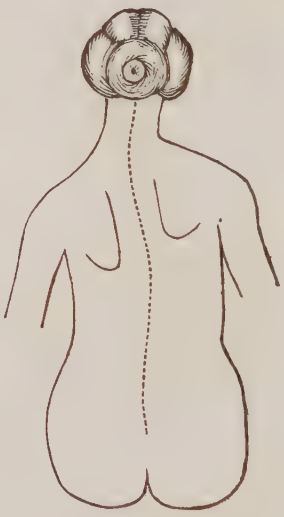


Fig. 6.

been able to represent in the drawing; but I give it as conveying an idea of deformity from muscular spasm. For all deformities, how much soever they may differ in particular details, have a characteristic aspect imparted to them by the inducing causes, which are never absent in any given type of cases.

I do not propose to weary the Academy with a description of every distinct variety of lateral distortion of the spinal column which is ever seen, nor to enter into extended particulars in regard to the pathology of even those forms already enumerated. My object will have been accomplished when I have called your

attention to those forms of spinal distortion depending on special causes. Although, taken together, they do not embrace a third of all curvatures of the spine, it should never be lost sight of that such cases do exist, and that it requires a clear and particular knowledge of their characteristics to be able to make a reliable diagnosis in any case of spinal deformity.

6. You will now be able to follow me with clearer appreciation in discussing the *legitimate* lateral curvature of the spine. I say legitimate, for the sake of greater distinction only. This class embraces not less than two-thirds of all cases, and comprises the only cases which are not produced by particular local causes. The origin of this deformity is simply the lack of sufficient muscular power to sustain the trunk at a period when there is also a want of firmness in the bones of the chest and spinal column. Mark well what I say. Muscular weakness alone will not produce a curvature of the spine. Let it occur at any time after the sixteenth year, and no lateral distortion will follow the longest-continued absence of the ordinary muscular support. Lateral curvatures of any forms do not originate after the sixteenth year, and, if previously formed, increase but slowly after that period. In fact, nearly all the first-mentioned forms of curvatures originate in early life; the last-named, the legitimate curvature, being latest of all to make its appearance.

Not often before the eleventh year, and more frequently during the twelfth or thirteenth year, a slight curve of the spine is observed, resembling somewhat that which is shown in figure 7 of the drawings. During the first stages it appears as often apparently toward one side as the other. When the young girl is brought to the physician, there not seldom arises a spirited dispute between mother and daughter as to which shoulder is the higher, or to which side the spine bends. The physician may be equally puzzled—he certainly will be if he has not studied these cases as they deserve—for at one visit the curvature and the prominent shoulder will appear on the side opposite to that which they were on the previous visit. At another time he may be surprised at not being able to find any curvature at all. This must be in the earlier stages. There is nothing wonderful in the fact that the curvature shifts from side to side, or temporarily disappears, at this

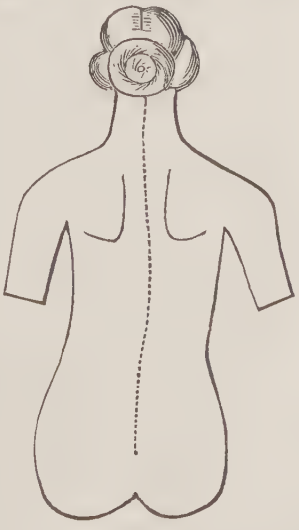


Fig. 7.

stage of the difficulty. The fact is, the curvature occurs only when the muscles are weak; and it appears toward that side where the most fatigued or weakest muscles are situated. If there are intervals when there is sufficient tone to the muscles, then at those times there will be no curvature. Or, if from various causes the tonic force of the muscles of the different sides of the body is made to vary, then the curvature must also vary.

After a while, the curvature is seen more frequently presenting toward one side, till, if the tendency is not arrested, it becomes habitually on one side. It is now a real curvature; before, it had only been a tendency to a curvature. This must all take place at a time when the osseous tissue is soft and impressible; a period lasting generally not over three years at the longest, and usually occupying a much shorter time. After a while—at fifteen or sixteen, sometimes a year or two earlier, but always coinciding with the period of individual development—there supervenes a more rigid state of the osseous tissue; the bones are no longer easily impressed, and the curvature ceases to rapidly increase, in most cases ceases to increase at all, and the case has arrived at the last stage, or the stage of complete formation of the curvature. There may in some cases be a slow increase of deformity once formed, especially if the patient be very weak and poorly nourished, but these are the exceptions and not the rule. As to the formation of a legitimate lateral curvature after the age of twenty or thirty years, it is simply impossible. I am often called upon by patients of the latter class, under the impression that a lateral curvature is just appearing. But I never failed to detect an error of this nature: the patient has been getting thin in flesh, and weak, perhaps accompanied with back-ache, when on examination a slight curvature is discovered. This discovery, under the impression that it has but just formed, is often the cause of great distress and alarm. Sometimes the patient will say that she had a curvature at fourteen, and was cured, but that it has reappeared. Now the truth is that the curvature did indeed form at the only age when this legitimate curvature can form; but that with the development of the womanly form and accumulation of adipose, the slight deformity was covered up and forgotten, to reappear when, with the rearing of children and advanced years, the rotundity disappears, and the curvature, which has existed all the time, comes to light again. I need not say that to connect the constitutional symptoms common to that period of life with a slight distortion of the spine which has existed all the time, during all degrees and phases of bodily health and bodily ailment, displays a lamentable want of knowledge.

It is at the age of puberty when the legitimate lateral curvature is formed. It is a deformity essentially peculiar to the higher civilizations. The other forms of lateral curvature we may find among all classes; but the one now under consideration is almost exclusively confined to the higher classes of society. Peculiar

combinations of circumstances are required in the production of this deformity, and we have the necessary conditions in greatest perfection among the higher classes. And these are the conditions: It is a period of the most rapid bodily growth, the vegetative processes are in greatest activity, and there is accumulation of quantity rather than firmness of tissue. Indeed, it often happens that the rapid increase of size is temporarily at the expense of strength; so that the strength and endurance of a fast-growing girl is less than it would be if she grew less rapidly, or than that of others of the same age of a slower growth. In a word, growth considered alone is an exhausting process; and very rapid growth may be, for the time, very exhausting. This we all see and know in the case of young animals. But with the young girl at this very period of rapid and often exhausting bodily growth, another process starts into controlling activity; ovulation and menstruation are establishing themselves. So overwhelming are these combined influences in controlling the girl's nature, that in the country, removed from the stimulating influences of city life—a forced intellectual effort which excites the mind to undue activity—there is at this time a period of very marked mental sluggishness. This is natural, and as it should be. It is well that nature has thus provided for complete and perfect development by temporarily subordinating mental activity to the increased demands of organic development. And if we did not thwart nature in these her conservative efforts by the habits of society, we should have none of this class of lateral curvatures.

But unfortunately, just at this period, when all the organic activities are in the completest exercise, the development of size, form, and the capacity to assume the maternal functions are demanding a paramount importance, and the functions of the brain and nervous system are naturally in temporary subordination, the habits of society step in to thwart this natural, easy, and successful process, by causing the brain to be in superior activity, and the organic processes to be held in subordination. The girl is allowed no period of intellectual sluggishness during which bodily development may become perfected. The nutrition which is destined for the body is diverted to the brain, and the bones and muscles starve. Hence people reared in cities do not attain, on an average, to the same physical stature of those reared in the country, nor are their bones and muscles so well developed in firmness and strength. So the muscles being deprived of both blood and innervation by the preponderating activity of the brain and nervous system, while the spinal column and all the osseous tissues are soft and yielding from the same cause, a curvature is the natural and inevitable consequence in a certain number of cases. The whole matter is simple enough. I have already remarked that these curvatures appear in the first stages on one side or the other, or temporarily disappear altogether, according to the accidental circumstance of partial, local, or complete temporary recovery of

muscular tone. But the muscular tone varies with that of the brain and nervous systems, and is entirely subordinated to it. So that in reality the large majority of lateral curvatures of the

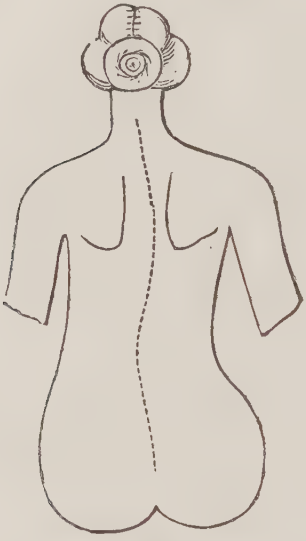


Fig. 8.

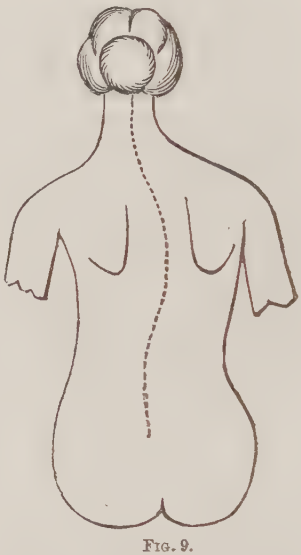


Fig. 9.

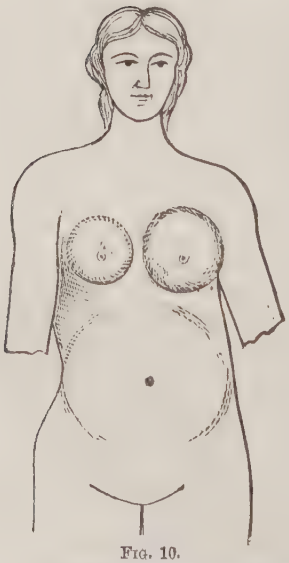


Fig. 10.

spine have their origin in the brain. It is the cumulative evidence of all my experience, that when we can control the action of the brain we immediately arrest, and if a state of mental repose is persisted in, we permanently cure this form of lateral curvature of the spine.

Figures 8 and 9 represent different cases of this curvature, after it has become permanently fixed towards one side; generally to the right. Figure 10 presents a front view of a rather severe case of lateral curvature, showing the usual marked difference in the development of the breasts. The breast on the flattened side of the chest is always much smaller than the one on the protruding side. I do not attempt to account for it. I present it as a singular and constant fact.

In insisting on recognizing the fact that lateral distortions of

the spine must be regarded, not as pathological conditions, nor as evidence of morbid change in the spine itself, but as deformities alone so far as the spine is concerned, depending on special local extraneous causes, or on a general combination of causes, at a particular stage of development, I would by no means underestimate its influence on the general health of the patient. But it is by having definite and correct ideas that we are enabled to apply remedies.

The lateral curvature is a deformity which diminishes the bodily vigor in many ways. The health is impaired, the form is rendered unsightly, and the patient, who is generally a female, has many of the pleasures of life destroyed by it. We cannot apply ourselves to a more worthy study, or one which will bring more joy to grateful hearts, than to seek to understand and remedy this all-pervading, spirit-crushing deformity.

On motion of Dr. Peaslee, Dr. Taylor was requested to continue the subject by illustrating the treatment of lateral curvature at some future meeting.

The Academy then adjourned.

Stated Meeting, April 1st, 1868. Dr. A. C. Post, President, in the Chair.

Aneurism of Subclavian Artery.

Dr. Finnell exhibited a pathological specimen, consisting of a huge aneurism of the subclavian artery (about the size of an adult head) taken from the body only a few hours before, death being caused this morning by the bursting of the tumor. The man was forty-seven years of age; the first symptoms noticed twelve years ago. Four years ago a pulsating tumor was first felt under the clavicle and increased in size from that time.

Dr. Charles F. Taylor continued the subject of "Lateral Curvature of the Spine," illustrating its treatment.

Treatment of Lateral Curvature of the Spine.

Mr. President:—Before entering upon the subject of the treatment of lateral curvature of the spine, which, in obedience to requests of the Academy, I now intend briefly to discuss, I desire a brief indulgence of the Academy while I make a passing allusion of great importance in the matter of diagnosis. It not unfrequently happens that cases present themselves with a lateral distortion, resembling more or less that represented in Fig. 11.

Now this illustration does not represent a true lateral curvature. It is an example of the usual lateral deviation which nearly always accompanies the first stages of Pott's disease, and is nothing more than an effort to relieve the spinal column of shock, and thus save the diseased vertebræ from injury. It is a temporary distor-

tion, always passing off with the progress of the case, towards either recovery or increased deformity, and it requires no treatment whatever.

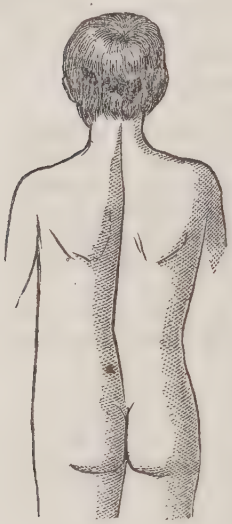


Fig. 11.

As a symptom of the first stages of Pott's disease, it is very important; but it is not a lateral curvature in the sense of a permanent deformity, and there should be care not to mistake the one for the other. I have known serious injury to result from recommending exercise and gymnastics to such cases, under the impression—which could only be the result of carelessness—that an ordinary lateral curvature was being prescribed for. It will be seen by referring to the figure (11), that this lateral distortion is not a *curve* after all, but an almost angular deviation to one side, terminating at some definite point of the spinal column, which portion of the spine will be found to be the point of disease. If there is a curve at some other point, it is simply a compensating curve. The only treatment this form of lateral distortion requires is that which is necessary for the cure of the diseased vertebræ. At best it is but a temporary incident in the course of Pott's disease.

In discussing the treatment of lateral curvature, I shall dismiss the numerous varieties with a few passing remarks at the close; confining what I shall say principally to the single but most common kind, which I have already designated by the term "legitimate" lateral curvature.

It will be remembered that we found that the most common form of lateral curvature occurs only at a certain period of existence; that this period embraces about three years, more or less, about the age of puberty; commencing a year or two before, and the liability continuing not later than the sixteenth year, and often ending much earlier; after which time the formation of the lateral curvature is impossible. I pointed out also that the immediate cause of this deformity is relaxation or weakness of the muscles at a time when all the tissues are in a soft and yielding stage of development; and I showed, at considerable length, that this weakness of the muscles occurring at this particular period is not due to any defect in the muscles themselves, but to several sources of *nervous* exhaustion acting at one time. These sources of nervous exhaustion were seen to be rapid bodily growth during the development of ovulation and menstruation; and the simultaneous abnormal activity of the brain and nervous system. And I stated the well-known fact that when persons grow up removed from mental excitements and stimulants—as of children reared in the country—

there was a very distinct epoch of mental sluggishness at the age of puberty. We then traced the *real* cause of lateral curvature, through the loss of muscular tone, to nervous exhaustion by reason of abnormal activity of brain at a certain period of development.

This being the pathology of these cases, the diagnostic inference is irresistible. Our treatment in the first or formation stage must have reference, not to the spine, but to the strengthening of the nervous tone by arresting the abnormal waste of force through the brain and nervous system. If we would arrest the formation of a deformity of the spine, we must diminish the action of the brain. We must stop the study and emulation; the excitements; the feverish irritability of the girl's brain and mind; we must, by all means, save her nervous strength. Let it never be forgotten that we are dealing with a case of *actual exhaustion;* and that the remedy for exhaustion is *rest.* We must be careful of muscular as well as of mental exercise. There is no truth in the idea that exercise of the muscles will compensate for weariness of brain. We cannot remove the effects of overdoing by extra labor. The constitution fails to recognize any difference in the avenues through which the vital force is let out. I repeat: overwork of brain is not repaired by overwork of muscles. When the system is exhausted we ought to recognize simply *that* fact, and at once secure rest as the only corrective of exhaustion, no matter how the exhaustion has been produced. Hence I must reject, as contrary to reason and all my experience, the idea of prescribing physical exercise for young, fast-growing girls, as a remedy for *incipient* lateral curvatures. The only remedy required to cure any ordinary case of this form of lateral curvature, in its first stages, *is absolute mental and physical repose.* They do not need exercise; they do not need apparatus; they do not need anything but to keep the brain inactive, the nervous system in repose, and the muscles still. Saving the nervous strength, which is usually wasted in premature activity of brain, is quite sufficient—*at this early stage*—to restore tone to the muscles, and thus to restore the form to the spine.

I may say in entire truth, that with all our large number of patients with this deformity, our very best cases, the most successful and the completest cures, do not receive any actual treatment whatever. If they can be made to obey directions, they are sure to recover. I am speaking of the first or formative stages. But a little later in the progress of the case, the circumstances become very much altered. The osseous tissues assume more firmness and begin to resist the action of the muscles. At this second stage a simple apparatus like the one figured in the 12th illustration will be found to answer a good purpose in assisting the muscles in their action against the spinal column. It is at this stage and afterward that the "localized movements," so called, for developing certain muscles, or groups of muscles, are found to be almost indispensable to complete success. It may

be still possible to cure the deformity without resort to any other mechanical treatment. With the assistance of this simple apparatus, the action of which must be apparent without further description, the spine is kept in position till the muscles, recovering their tope, are able to hold it erect without assistance. But as time tasses, with the deformity not removed, the muscles do indeed recover their tone, but the osseous tissues have also become rigid and fixed in their distorted position, and we are confronted with a deformity which might have been easily prevented, which might even have been removed a little time before; but which now will

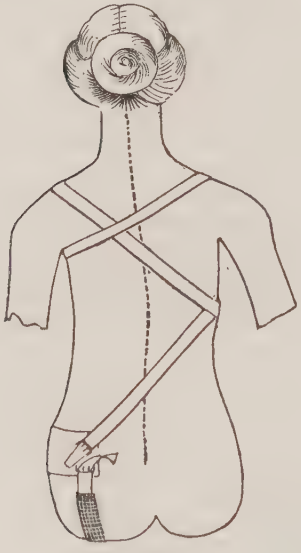


Fig. 12.

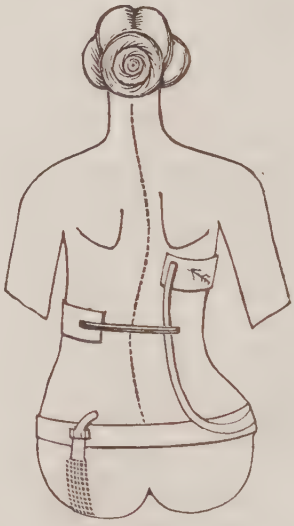


Fig. 13.

tax all our resources to modify and mitigate, but which we seldom hope to *entirely* cure, as we could have done a little earlier. We can do much to relieve at any stage; we can diminish and ameliorate; but the precious months have passed unimproved during which we could have saved her form, and sent the young girl out into the world in *perfect* symmetry.

From this moment we are confronted with an absolute *deformity*, resisting reduction, and against which *force* must be used. We have two means through which we may endeavor to accomplish the reduction of the deformity. We may endeavor to diminish the distortion by apparatus which act with sufficient force against it; and then we strive to retain the spine in a more perfect position by increasing the power of the various muscles which act upon it.

Figs. 13 and 14 represent different varieties of apparatus used

for the legitimate lateral curvature; while figure 15 shows one form which is very efficient in some of the other forms of curvature, especially where the distortion is high between the shoulders. These instruments are intended to effect a certain definite object,

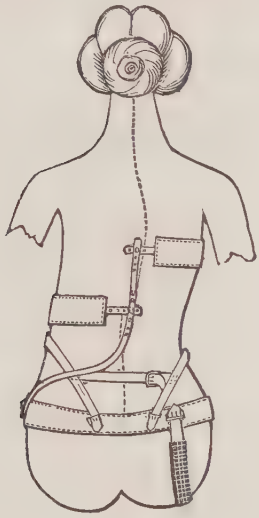


Fig. 14.

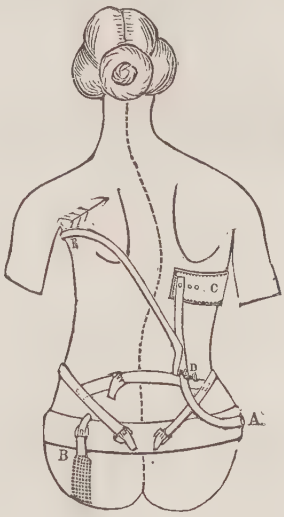


Fig. 15.

and they are made with such an action as will accomplish that object. They are made strong enough, so that, acting in a certain

Fig. 16.

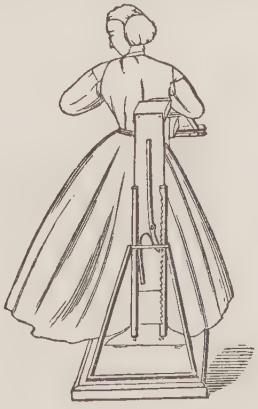


Fig. 17.

direction against the spinal column, the latter and not the instrument must yield. They are perfectly adjustable, and are made

to reduce rather than to cover up and hide the deformity. And it is astonishing what wonderful results may often be obtained in apparently the worst cases. I speak with entire exactness. While it is certainly true that *perfection* of form can scarcely be hoped for after the case has passed into the second or rigid stage, yet we

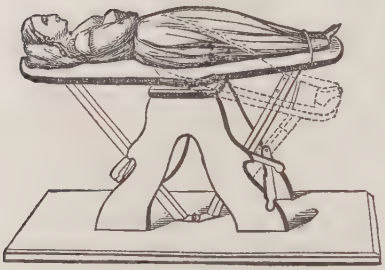


FIG. 18.

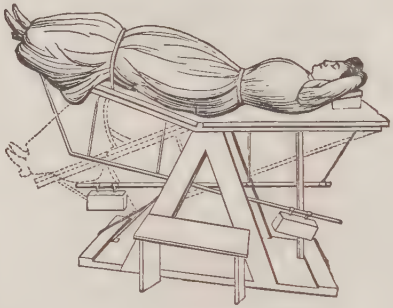


FIG. 19.

can often almost attain it in a large proportion of cases by perseverance in the *right* direction.

Acting in the same direction and on the same mechanical principles, but still more powerfully, are the stationary apparatus,

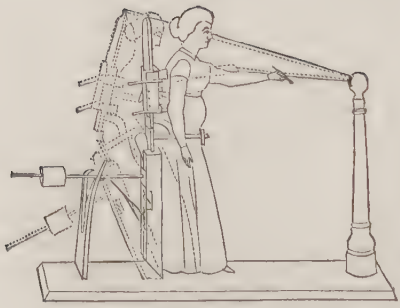


FIG. 20.

FIG. 21.

illustrated in figures 16 and 17, while figures 18, 19, and 20 are intended to represent some means of securing the local exercise

of certain muscles, or groups of muscles. This is a necessary part of the treatment in all cases of lateral curvature, after the incipient stage is passed. The loss of harmony in the action of the muscles induced by malposition must be overcome by direct local regulation of the action. Not only the bony framework must be restored, as near as may be possible, to its natural shape, but the muscles must be restored to their normal action. To strengthen them where weak and relaxed, and to develop and extend where shortened and atrophied, involves a system of exercise or "movements" for the purpose. Several illustrations are given.

The exercise illustrated in Fig. 18 is calculated to strengthen the muscles of either or both lateral parts of the trunk, while Fig. 19 shows how the spinal muscles alone may be put into separate action or exercise. Fig. 20 represents the method of extending the contracted muscles of one side in certain forms of lateral distortion, while at the same time the spinal column is extended, as a glance at the figure will show. Fig. 21 represents a complicated action of different portions of the spinal muscles, at the same time. I may be allowed to remark that these last figures serve as examples of what is sometimes called *localized movements.*

I shall not weary the attention of the Academy by entering into minute details of treatment, deeming it only necessary to suggest general principles; the special applications of these general principles constitute the *tact* of experience. I may remark that the apparatus used always contemplates purely *lateral* action, never longitudinal extension; and that an important and characteristic feature of the instruments is, they are levers deriving their power from a perineal strap, which *secures a definite unchangeable point of action* for whatever force it may be necessary to employ. The apparatus on the table and the numerous drawings will sufficiently suggest the main ideas of the treatment, without further verbal illustration. I do not intend to weary you with minute descriptions of what ought to be sufficiently obvious without.

With regard to the numerous varieties of the least common forms of lateral distortion, the treatment is more or less successful, according to the circumstances of each case; but these exceptional cases are not those in which the general practitioner will be likely to have much success, should he undertake them. And even the legitimate lateral curvature, after it has become a fixed deformity, will be likely to resist all but very vigorous attempts at reduction. It is only at the initial stage of this deformity that the general practitioner will find himself sustaining responsibilities which he is capable of discharging, and these he can discharge with the most perfect success. But after the first stage the treatment requires more facilities than he generally has at his command.

I commend, therefore, these views of the pathology and treatment of lateral curvature of the spine to the serious consideration

of every conscientious physician, believing that if faithfully carried out they will be the means, with others as with me, of saving many persons from deformity and distress, as well as being the source of much personal satisfaction to both patient and practitioner.

The Academy then adjourned.

STATED MEETING, APRIL 16TH, 1868. DR. A. C. POST, PRESIDENT, IN THE CHAIR.

DR. GEORGE E. POST, of Beirût, read the following paper:

THE DISEASES OF SYRIA, AND A SKETCH OF THE PRESENT STATE OF MEDICAL PRACTICE IN THAT COUNTRY.

Before proceeding to a view of the diseases of any country, it is proper to inquire into its geographical features and peculiarities of climate, with attention to the race and physiological attributes of its inhabitants, and the modifications which these natural attributes have undergone owing to social habits and religious belief. Accordingly, I shall preface my remarks on the diseases of Syria by a brief elucidation of these features of the country and its people.

Syria is a geographical expression for that portion of the Turkish empire included between Mount Taurus on the north, and the desert of Idumea on the south; and between the Mediterranean sea on the west, and the plains of Mesopotamia on the east.

The physical geography of this region is strongly marked.

If the eye be cast upon a physical map of Syria, the glance is arrested at a point a little inland from the mouth of the Orontes by a mountain chain, which runs southward, parallel with the Mediterranean coast, and at a variable distance from it, towering highest at about the middle of its course, between Tripoli and Beirût, and thence subsiding gradually until it is broken into the rugged, sterile desert, stretching away to Petra. This mountain chain, 350 miles in length, though separated in descriptive geography into various lesser ranges, known each by its special name, as Mt. Cassius (Jebel Okrá), Nusairûjeh Mountains, Mt. Lebanon, and the hill-countries of Galilee, Samaria, and Judea, is in effect a single range, marked by the same geological structure, which is the Jurassic limestone, abounding in fossil-shells and fish, and presenting at places gigantic out-croppings of ferruginous sandstone, in which are found small seams of coal. This chain is broken at two points by deep and broad valleys, connecting Cœle-Syria with the sea-coast. The northernmost of these is the plain which enters between the range of the Nusairûjeh Mountains and Lebanon, and reaches to the northern outlet of Cœle-Syria, and the edge of the Arabian Desert at Hums, while the southernmost

divides Lebanon from Palestine, and terminates at the western foot of Hermon.

There lies between this mountain chain and the coast a narrow strip of plain, expanding above Tripoli into the prairie of Akkar, and near Joffa into the plain of Sharon. From this plain the mountain rises by terraced slopes to a height varying from 2,500 to 10,000 feet. The descent from the summit ridge toward the east is more precipitous, and its eastern foot is bounded for the northern half by the broad valley of Cœle-Syria, and southward by the narrow bottom-land of the Jordan. Cœle-Syria is elevated about 1,500 feet above the sea, but the valley of the Jordan gradually descends from this elevation until at the mouth of the river it is 1,300 feet below the surface of the Mediterranean, with a climate and flora like that of the equatorial regions.

This plain and river-bottom divide Lebanon and its continuous ranges from a parallel range, known in its northern half as Anti-Lebanon, and in its southern portions as Gilead or Bashan, Nebo, and the Mountains of Moab. The highest peak of this range, Mt. Hermon, 10,000 feet in height, overlooks the sources of the Jordan on the west, and Damascus on the east, beyond whose fertile meadow the eye strains to find a boundary to the great plain which stretches away to Arabia and Persia.

It will be readily understood that these vast ranges of mountains, rising like two enormous backbones from the seacoast on the one hand, and the illimitable desert on the other, must impress upon the climate of Syria some peculiar features. The prevailing winds, during the later spring and summer months, are from the west, and while sufficiently charged with moisture to impart a refreshing sensation, are never accompanied by any considerable amount of rain. From April to November, although the sky is cloudy, often for days together, there is seldom even a sprinkle of rain. In October and November, however, the wind begins to set in from the S.W. and S.S.W. A glance at the map will show that this wind must blow over the African Desert. When it leaves the northern coast of Africa it is hot and dry to the last extreme; immediately it drinks in copiously of the water of the sea, and soon becomes surcharged with vapor. When it reaches the Syrian coast it mounts the lofty peaks of Lebanon, and encountering the stratum of air, chilled by the snowdrifts of that elevated region, its moisture is precipitated in abundant rains. These S.W. winds prevail from the latter half of November to the latter half of March, and it is during this short period of four months that all the rain of the year occurs. Snow-banks many yards in thickness are piled on the summits of Lebanon and Hermon; the springs are replenished; dry wadies flow with unaccustomed torrents; and the plains become verdant with a semi-tropical vegetation.

In the spring the winds shift to the eastward. Blowing as they must, when coming from that quarter, over the dry steppes of Persia, and the great waterless plain east of Damascus, they are

as dry as those from the Sahara. In the early part of the season, they are cold, though the sensation which they cause is deceptive, owing to the rapid evaporation from the surface of the body. The temperature seldom goes far below the freezing point. Later in the season they become warmed by radiation from the great plains over which they blow, and retaining their character of extreme dryness, they feel like the blast of a furnace, parching the skin, and causing the leaves to droop. The whole body becomes relaxed, and the appetite and digestion are affected. These winds are termed Sirocco. They not unfrequently last for weeks together. Great relief is experienced when they give place in June to the balmy west wind from the Mediterranean, notwithstanding the blazing sun which then rules the cloudless sky.

The temperature of Syria is marked by great evenness, and the absence of extremes both of heat and cold, more especially the latter. The thermometer at Beirût, in the latitude of 34°, parallel of Wilmington, N. C., rarely rises above 95° Fahrenheit in the shade, and at the same hour for the successive days of the month will not vary more than two or three degrees. Its range during the twenty-four hours hardly varies ten degrees Fahrenheit in summer. One can sleep at night out of doors, during that season, with almost as little covering as he would require for a nap in the shade during the day.

Snow is almost unheard of on the sea-coast plain, and below 2,000 ft. on the seaward water-shed of Lebanon. It becomes abundant, however, at an elevation of 4,000 ft., and lies from one winter to another in immense drifts on the higher peaks. The great road from Beirût to Demascus, which crosses the summit ridge of Lebanon at an elevation of about 4,500 feet, was so obstructed by snow during the past winter (1867–8) that the mail-coach was stopped for three consecutive weeks.

Syria is naturally subdivided into three climatic regions.

I. The sea-coast plain with the lower slopes of the mountains, and the lower portions of the valleys of the Orontes and Leontes near their mouths. This region has a sub-tropical climate, perpetual verdure, is free from snow, and is marked by a characteristic vegetation. Here flourish the palm and the orange, the banana and the citron, the sugar-cane and the henna plant.

II. The second region includes the middle zone of Lebanon, with the corresponding elevations of the lower ridge, and much of the valley of Cœle-Syria. This region is like the warmer parts of the United States in climate; snow falls in moderate quantity in winter, and the vegetation assumes a different character. Among the trees of this region are the willow and the oak, while its herbaceous flora resembles the types of Southern Europe.

III. The third region, which includes the northern portion of Cœle-Syria and the plain east of Damascus, approaches in character the great waterless plains of Persia. Its vegetation is reduced to a few stunted thorny shrubs of the Compositæ and Leguminosæ,

with some species of Rubiaceæ and Labiatæ, and its broad prairies are covered with grasses and sedges in the spring, furnishing pasturage to the vast flocks and herds of the Arabs.

The following is

A Thermometric Record kept at the Prussian Hospital at Beirût, 1866 A.D. *The figures are the mean of seven observations a day, taken both in the sun and shade. The thermometer used was marked with the Centigrade scale.*

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sep.	Oct.	Nov.	Dec.
1	11.4	12.9	14.9	16.5	20.0	24.4	27.6	28.6	28.0	25.9	22.1	17.6
2	9.4	12.4	13.7	17.6	20.5	26.6	27.9	29.4	27.7	25.9	20.7	16.4
3	14.9	14.0	14.5	16.7	20.7	25.2	28.2	29.4	27.7	24.2	19.6	16.7
4	13.6	13.1	15.6	17.7	22.0	24.7	27.9	28.7	27.0	24.7	19.4	15.5
5	13.7	13.6	14.7	18.2	22.4	24.6	28.0	29.6	27.4	24.6	20.2	14.9
6	13.1	14.1	16.4	17.4	21.9	24.5	28.5	28.5	27.4	24.2	19.1	14.7
7	13.6	15.5	18.6	17.9	21.9	24.0	28.6	28.9	27.9	24.5	20.6	16.9
8	12 9	14.6	18.5	17.7	23.6	23.2	28.1	28.9	27.0	24.5	—	15.1
9	13.4	14.6	16.7	18.2	22.9	24.0	29.1	29.0	26.7	25.6	20.0	16.1
10	11.7	14.6	16.6	19.4	23.4	24.6	29.1	29.5	26.6	26.1	20.2	15.7
11	14.6	14.0	15.7	20.7	22.6	25.4	29.9	29.2	26.7	26.1	20.7	15.4
12	13.5	14.0	16.7	20.4	22.6	25.1	29.6	29.2	26.6	25.0	18.7	16.6
13	11.9	15:5	16.9	19.7	23.1	25.1	29.1	28.9	26.6	25.6	19.4	16.5
14	13.4	15.6	14.7	19.1	23.2	25.2	29.1	28.2	27.0	24.5	20.0	15.5
15	14.1	15.4	17.1	19.6	24.0	24.5	29.4	28.2	26.7	24.2	19.4	17.0
16	12.4	14.0	17.5	19.1	23.5	25.2	29.4	28.0	26.4	25.2	18.9	16.6
17	11.5	14.2	15.7	18.2	23.1	25.5	29.5	28.1	26.6	26.0	19.6	16.0
18	12.4	13.7	16.6	19.9	22.5	25.6	29.1	27.1	26.6	24.6	17.7	15.4
19	12.2	12.9	16.7	21.6	22.6	26.5	28.6	27.5	27.0	23.7	18.7	16.7
20	13.1	11.6	16.2	20.2	22.5	25.7	28.5	27.7	26.4	22.0	17.2	16.6
21	12.9	13.9	17.2	20.4	22.5	26.1	29.0	28.0	26.6	19.6	18.4	17.0
22	11.6	15.4	21.4	19.4	21.7	26.1	29.1	27.9	26.9	19.7	17.7	16.0
23	11.9	16.5	21.7	20.5	21.5	26.9	29.7	27.9	26.5	18.5	18.2	—
24	12.5	15.5	22.1	20.6	22.1	26.5	29.1	27.7	25.6	18.6	19.6	—
25	14.5	14.7	21.5	20.9	33.1	26.5	28.4	27.2	25.2	18.6	13.7	—
26	14.0	20.5	18.5	21.2	24.2	27.2	29.1	26.9	25.4	20.4	12.9	—
27	14.1	17.2	18.0	20.1	24.0	27.1	29.9	26.9	25.4	21.2	15.0	15.9
28	14.9	15.5	22.2	19.9	24.6	27.2	28.9	28.0	25.7	21.9	17.6	15.6
29	15.6	—	18.1	19.7	25.0	27.4	28.7	27.9	26.0	21.9	17.2	16 1
30	14.6	—	17.0	19.7	24.9	27.4	27.9	27.1	26.1	22.4	18.5	15.9
31	12.6	—	17.9	—	25.0	—	28.1	27.6	—	23.0	17.0	15.6
Average for the month.	13.1	14.6	17.4	19.2	22.7	25.6	28.7	28.2	26.6	23.2	18.5	16.1

(IV.—A fourth region deserves notice in passing—I allude to the Alpine summits of Lebanon and Hermon, with their enduring snows and characteristic vegetation, among which is one plant of the strictly *arctic* flora.)

From these geographical and climatic peculiarities, it might be inferred that Syria would present great variety in the types of disease, as will appear somewhat later.

The people of Syria are a mixed race. The aborigines of the soil were a Shemitic people, coming, in all probability, from Mesopotamia. The Hebrews, another Shemitic race, but with peculiar laws and institutions, displaced this ancient race in some sections, and in others became fused with them. Into this oriental nation, the blood of the Indo-Germanic race was transfused to a limited extent by the Greek and Roman conquests, though the barriers of religious prescription, then as now, prevented intermarriage of persons of opposing sects, and thus hindered the mingling of new blood.

The Arabian conquest, with the subsequent conversion of many of the *men* to Islam, and the absorption of many of the women into the hareems of their conquerors, fused the Moslem portion of the population into a homogeneous type. But those of the people who retained their religions remained of a separate physiognomy, and there resulted the anomaly of four permanent types of mankind in this small section of the world; the Arabic, modified by intermarriage with all the rest; the Maronite, representing the aboriginal characteristics; the Greeks, representing the Indo-Germanic graft on the original stock; and the Jews, representing a peculiar phase of the Shemitic family. To this day the subdivision of Oriental cities into quarters for the exclusive residence of these various sects, is but a faint symbol of the division of sentiment and habits which characterizes their several races.

Yet these diverse races agree in many important traits and habits. They are all a highly contemplative people, inclining to sedentary pursuits. They even prosecute their trades and professions in the sitting posture. They are naturally indolent, and all smoke incessantly. A Syrian cannot be properly drawn without the accompaniment of his long pipe or bubbling argôleh. Their various religious systems, while differing in doctrines and rites, agree in prescribing seasons of rigid and long-continued fasting, and allow and encourage subsequent feasting to an extent highly prejudicial to health.

The Moslem faith requires every believer to fast all the days of a whole month. No food, drink, or smoke must enter the mouth, from the time when "one can distinguish between a white thread and a black until the setting of the sun." The rich spend the day in sleep, and the night in revelling. The poor work as usual during the day. The porters on the beach, at the maritime cities, wade into the sea up to their necks, and carry prodigious burdens all day long to the shore, under the burning sun, yet not even a drop of water may pass their parched lips. When sunset comes they plunge into the most reckless indulgence of appetite, and often into the most excessive venereal pleasures.

The Christian fasts, though less rigid, are scarcely less injurious; inasmuch as during the forty days of Lent, and other less protracted fasts scattered through the year, they abstain from all forms of animal food, even butter and milk, and consume incredi-

ble amounts of oil, onions, garlic, salad, and other innutritious and unwholesome articles of diet.

After the termination of these fasts, the feasts are most intemperate as regards eating, though intoxication is happily rare.

The diseases of the country may be inferred from what has been said of the race and habits of the people, and the climate and physical peculiarities of the country.

In the way of exclusion, we remark on the rarity of all diseases of the larynx, and bronchial tubes, and parenchyma of the lungs. Tubercular consumption is rare, being found mostly in persons of depraved habit of system, and for the most part it is sporadic, not characterizing whole families as with us.

Diphtheria and diphtheritic croups are almost wholly unknown. I have heard of but one authenticated case during my residence in Syria. I have never seen one.

Scarlatina must be very rare, as it has not even a name in the vernacular Arabic. This disease, if it occur, must be very mild in form, and attract little attention. I have not seen a case.

Measles, which in our climate is apt to be followed by disastrous sequelæ, due to the effect of cold, affecting more particularly the bronchial apparatus, is there more apt to be complicated by infiltration into the meningeal cavities, or by abdominal disorders.

One of the most common diseases of the country is dyspepsia. This results in part from the gross modes of cooking universally adopted. Eggs are usually fried in twice their weight of butter or oil, which is eaten with them. Onions fried in oil, until they are encased as in a coat of mail, with bits of meat cooked in oil or butter until they are hopelessly indigestible, dried wheat cracked and boiled, then drenched in butter, are a few of the commonest articles of diet. Fruit is eaten in inordinate quantities, and, by a singular perversion of taste, the natives prefer unripe melons, grapes, and figs, to those which have reached the perfection of their development. Almonds in the undeveloped state, when the husk is still soft, in the state in which we select walnuts and butternuts for pickling, are eaten husk and all, with salt in the early spring. Green and hard apricots are eaten in a similar manner. Clabber is a universal article of food. But the times of eating are more injurious than the mere articles of diet. The laboring classes go to their work at an early hour in the day on an empty stomach, and work until near noon before they eat. Their breakfast is light, and not until after sunset do they eat a hearty meal. This meal is usually very solid, and proportioned to their accumulated hunger. They have a great fondness for salt and high seasoning, and consequently drink copiously of water. They then retire immediately, before digestion has been accomplished, and sleep heavily.

This regimen produces its legitimate effects in wide-spread and exaggerated forms of dyspepsia. Almost every one has more or less of this disease.

The sedentary habits of the people induce *hæmorrhoids*, which afflict a large proportion of both sexes. The effect of such a large proportion of oleaginous food, which at first would seem to tend to laxity of the bowels, in the end produces constipation, which acts as a cause and aggravation of the piles. A still further exciting cause of this disorder is the mode of equitation. The saddle consists of a broad level platform stuffed with dry grass, which becomes matted and hard. This causes the legs to be widely abducted, and the anus to be irritated by the contact and friction of the hard saddle, and as no stirrups are used by the common people, the whole weight rests on the perinæum. And inasmuch as the only mode of travelling in that country is on the backs of beasts of burden, these causes operate far more generally there than here.

Diarrhœa, dysentery, and other abdominal disorders are frequent and fatal. They result from the excessive use of fruit,* especially in the green state, and the other dietetic imprudences alluded to, coupled with the steady relaxing heat of summer, and the almost mephitic exhalations of the narrow, filthy streets. An Oriental street has been appropriately described as a receptacle for all refuse. In many of the smaller towns of Lebanon the drains of the privies are turned upon the public streets, and the decaying fæcal matters run down the channel which the rains have worn in the middle of the way. When the sun shines hot upon these putrid masses, the odor is ineffably disgusting. In this filthy atmosphere the degraded poor, nay, many even of the richer classes perpetually live. When the cholera breaks out in these localities its ravages are frightful.

It might be expected, from the long continued heat and relaxing character of the climate, that these diseases should be especially fatal with children. This is certainly the case to an alarming extent. Births, owing to early and universal marriages, and intense desire for offspring, are probably more frequent in Syria than in America. But the mortality among infants is immense.

Calculus is rather a common disease, especially in some districts where the water is strongly impregnated with mineral constituents.†

But among the characteristic diseases of Syria, none are more interesting than the various ophthalmiæ. The dry air of spring and early summer, with the clouds of dust, filled with all imaginable filth of the most irritating character, and bearing in its current innumerable flies, produces the most virulent forms of purulent ophthalmia. These left, as they generally are, without treatment, or trifled with by the employment of inadequate means,

* I have known a man to eat a hundred Indian figs at a sitting. He died the same day of epidemic cholera, which was then prevailing.

† Syphilis prevails to a large extent, especially the secondary forms, which are propagated by the practice of smoking in common from a single pipe, which prevails at their social gatherings.

result in granular lids, which are aggravated by smoky houses, and the continued action of the irritating dust and glare of the sun, so that ectropium invariably ensues, and not a few cases are seen where the eye is surrounded by a velvety injected ring of tumid florid conjunctiva, three-quarters of an inch to an inch in breadth, and the cheek becomes excoriated and ulcerated by the epiphora of the tears.

The whole catalogue of sequelæ of conjunctivitis is amply illustrated in every clinique in Syria. Ulceration, and total destruction of the cornea, staphyloma, and total disorganization of the globe of the eye, are the commonest of spectacles. Dr. Van Dyck, of Beirût, estimated that over a thousand persons lost one or both eyes during an epidemic of purulent ophthalmia which prevailed in that city last year—1867. Ear diseases, however, owing to the mildness of the climate, occur in far smaller proportion, and are not among the characteristic disorders of the country.

The deeper-seated inflammations of the eye are not specially common in Syria, but owing to the total ignorance prevailing in respect to them, they are far more disastrous than with us. And as there is not a spectacle-maker in Syria, among the native population, there is little to alleviate the inconveniences of short or long sight.

Diseases of the skin are very common and various. Owing to the uncleanly habits of the people, and the impossibility of regulating their diet, they are a most intractable and inconvenient form of ailments. Leprosy still exists, but is not very common. It is incurable.

Among the female disorders are dysmenorrhœa and amenorrhœa, resulting from their sedentary habits. Procidentia and prolapsus uteri are common among the poor, owing to the early return of parturient women to their usual work after child-birth. A washerwoman, whom I employed in Tripoli, gave birth to a child on Sunday, and started for my house on Tuesday to do a day's washing. I, of course, remanded her to her bed.

Ulceration of the neck of the uterus is exceedingly common. The well-known desire of Oriental women for offspring, when not gratified in the usual way, leads both husband and wife to resort to the most empirical means of promoting fecundity. Men take the stimulating diuretics, and eat those articles of food reputed for their erotic and fecundating tendency. Women, beside the use of charms and incantations, and the use of articles of diet of the class mentioned, have great confidence in the virtues of sitting astride the tombs of the prophets and saints, and the visits to certain celebrated shrines. But the practice which prevails most universally among them is the introduction of various irritating substances into the vagina and rectum, with a view of promoting sexual desire, and increasing the flow of the menses, or bring on leucorrhœa, which is supposed to have a beneficial tendency. An instance has been known where the eggs of a serpent were introduced into the

rectum of a woman, and the reptiles hatched out in the genial warmth of that receptacle. It is not surprising that, with the prevalence of such malpractice, ulcerations of the neck of the uterus should be common.

Small-pox still prevails, epidemically at times, although vaccination has been generally introduced of late years. Still, as it is not performed by physicians, it is often neglected, and oftener imperfectly performed.* The last class of diseases to which I will allude is the malarious fevers.

Owing to the small quantity of rain which falls during the summer months, cultivation is carried on by the process of irrigation. The mountains are terraced to the limits of soil, and the water of the fountains is preserved in tanks, from which it is daily let out over the fields and gardens. The neighborhood of the cities on the plains is more especially the seat of this kind of cultivation. Damascus, Beirût, Tripoli, Sidon, and Hanrath, are surrounded by extensive orchards of orange, lemon, apricot, mulberry, peach, and plum trees, and by gardens in which every variety of vegetable is raised in the greatest profusion, and of the finest quality. Streams of water run by every hedge, and night and morning these streams are distributed in little canals and furrows to every foot of the fertile loam, over which is spread the night-soil from the city as a fertilizer. Thus every inhabited place becomes as it were an extensive swamp, a nidus of malignant malarial poison. Few even of the natives, who remain in the cities during the months of August, September, and October, escape intermittent or remittent disorders, and even those who do not have fully developed agues, suffer from indigestion, neuralgia, disorders of the liver, and other results traceable to the malarious cause. Those, however, who

* Typhus and typhoid fevers are common, and annually destroy multitudes, especially in the crowded, filthy cities. In the East the principles of good nursing, with the preparation of an appropriate diet and regimen for the sick, are quite unknown. When a man is taken sick, his relatives and friends crowd into his chamber, to express their sympathy, and to cheer and strengthen his spirit to a resolution to recover. This ill-judged friendship, in addition to its tendency to vitiate the air of the room, prevents sleep, excites the patient, and hinders the performance of his vital functions. I have known cases in which a patient just subsiding into the repose of a much needed slumber, after the excitement of a sleepless night, has been awakened by a well-meaning friend in order to assure him that he was going to sleep, and not dying. The critical moment of disease is thus often made the moment when the fate is sealed, and the hope of life cruelly and causelessly snatched away. In no class of disorders is this more true than in continued fevers. Again, the necessarily protracted character of these fevers induces impatience in the management of them, and a disposition to meddle unduly with the processes of nature by useless treatment, the more injurious, as the long duration of the disease gives opportunity for greater variety and incompatibility in the means used for recovery. Every visitor recommends a fresh abstraction of blood, or a new cathartic, and none realize that their absence, with the employment of simple means of nourishment, and attention to the wants of the system as they arise, would do more for the patient than their lancets and their nostrums.

spend the later summer and early autumn months in the mountains, escape this danger.

The liability of the people to these disorders is greatly aggravated by their habit of sleeping out of doors, on the low, flat roofs of their houses, often by the sides of the canals, in the gardens, during the pestilential months. They are driven to this course by the ill ventilated condition of the close houses and streets.

The so-called *Syrian fever*, is a remittent, of a low typhoid type, marked with extreme prostration, and congestion of the membranes of the brain. It is characterized by marked sleeplessness, a feature by which the gravity of the attack may be accurately gauged. This symptom does not yield readily to narcotics, but calls for heroic doses of quinine. I have myself administered two drachms in twenty-four hours in a case where no sleep had been procured by natural or remedial means for four days. The second dose of fifteen grains, given three hours after the first, induced sleep, which proved the turning point of the disease. Dr. Barclay informed me of a case where four drachms were given in corresponding circumstances with the happiest effect.*

Before proceeding to my remarks on the present state of medical practice in Syria, I will observe that the peculiarities of diet of the East render the operation of cathartics less certain than with us. I have given twenty drachms of castor oil on two successive mornings to a man, who informed me that the only result was colicky pain in the abdomen, and a single passage. Fifteen drachms of crude castor oil is a standard dose.

The Oriental constitution is peculiarly susceptible to the action of mercurials. Five grains of blue-pill, administered as a cathartic, will sometimes produce serious salivation.†

Wherever disease or pain exist, man is instinctively impelled to use remedial measures, and in every community, however uncivilized, we find a class of persons reputed for their skill in the practice of the art of healing.

We might expect, from the history of the Arab race, and their eminent attainments in the science and art of medicine, that there would have remained schools, representing at least the degree of skill attained by Avicenna, and a class of practitioners conversant with those truths and theories which prevailed in his day. But science and art, when they cease to be progressive, enter at once

* Death not unfrequently succeeds promptly the first access of one of these congestive forms of malarious disease, when the congestion affects the stomach or brain. In the former case the sudden death may be attributable to shock communicated to the sympathetic system by means of the solar plexus and semilunar ganglion.

† Syria is particularly rich in articles of the Materia Medica. Among the more important indigenous products in this department are the Sponge, Leech, Poppy Colocynth, Scammony, Galls, Conium Maculatum, Saffron, Hyoscyamus, Ricinus, Laurus Nobilis, Aloe, Squill, Lemon, Liquorice, Senna, Rue, Elaterium, and Tragacanth, beside many others of approved virtues and importance.

upon their decline. Greece, which once produced the models which are still the norm of sculpture, poetry, and oratory, has no longer a sculptor, nor a poet, nor an orator. So when the masters of medicine in the Arabian Universities, at whose feet all Europe sat and learned, had passed away—when the spirit of investigation and experiment ceased from its activity, the Arabs not only lost what had been gained by the painful and laborious research of their own ancestors, but in their pride refused to learn of those whom they regarded as infidels and upstarts in science. And in our own day, when medical science has achieved so proud a position in Europe and America, the Arab race has lost all that it possessed centuries ago, so that not a school of medicine remains among them to keep alive even the name of the profession once so honorable and illustrious.

Yet there are not wanting among the Arabs those who lay claim to medical knowledge and skill. The works of the fathers have been transmitted in fragmentary copies, in manuscript, from generation to generation, and form the basis of what remains of knowledge of disease and its treatment. The name of Hakeem (wise man) is one of great dignity, being the embodiment of the idea of complete knowledge and skill. The pretender to this knowledge is still supposed to know by intuitive perception the character of disease. He can tell by feeling the pulse whether a woman is pregnant, and if so whether her fœtus be a male or female. And by his Theriaca, a compound of seventy-two sovereign ingredients, he can subdue the most obstinate diseases. Such is the traditionary respect for the profession, that its honors are still given to the impudent charlatans who are its sole living representatives.

Even those who are best read in the writings of the Arabian physicians can of course know little or nothing of large classes of disorders, the knowledge of which is the fruit of modern research.

Diseases of the brain and nervous system are attributed to the influence of the Genii. A child in convulsions is supposed to be touched by the evil one, and for these disorders they employ exorcisms and amulets.

Most of the diseases of the eye are confounded under the generic term *Ramad*, signifying ophthalmia, and no attention is paid to the anatomical seat of the disorder. In particular, they fail to distinguish the various forms of conjunctivitis from iritis and inflammations of the sclerotic, choroid, and retina. Purulent conjunctivitis is let alone until "the eye is ripe," that is until the mischief is all over. No attempt is made during the acute stage to open the lids, cleanse the ball of the eye from pus, or use any measures to arrest the inflammation, or relieve the *chemosis*. The most that is attempted is general depletion by the lancet and cathartics, and the local application of leeches.

Inasmuch as they are unacquainted with both the normal and pathological anatomy of the internal eye, no special treatment

is known for the class of internal inflammations, with their resultant pathological lesions. Iridectomy is unknown. The use of mercury and iodide of potassium in iritic troubles is quite beyond the teachings of their science. The operations of depression and reclination for cataract are performed by Persian specialists, but extraction is unknown. The vast number of eyeless orbits, and staphylomata, and iritic adhesions, and aggravated cases of granular lids and ophthalmia tarsi, show the extreme ignorance which prevails in all diseases of the eye.

In aural disorders there is absolute ignorance. They make no attempt either to distinguish or treat them, save by a general application of the poultice, and narcotics to relieve pain. A similar remark applies to the diseases of the nares, larynx, and fauces. Excepting the use of some astringent gargles, and abstinence from certain articles of food, nothing is attempted in the way of relief for this class of diseases, which is happily infrequent.

Diseases of the chest are known only by rational signs, and are treated by the most violent antiphlogistic means. I have known of repeated bloodlettings being practised for the hectic of phthisis. The rule is to bleed the patient; if he get no better, bleed him again, with the addition of leeches and cathartics. This is repeated until the patient recovers or dies.

Of the numerous abdominal disorders few are understood. In diarrhœa they exercise little restraint on the diet of the patient, save in conformity to the artificial distinctions of the older physicians between the *warm* and *cold*, i. e., *heating* and *cooling* food. Cucumbers and melons are allowed to patients suffering from diarrhœa and dysentery, and nothing which the disordered appetite of the patient may demand is denied to him. Green almonds, and apricots, and the pulp of lemons are favorite luxuries in such cases.

The cure of hæmorrhoids is attempted by the use of astringent and cooling lotions, but operations on the rectum and anus are unknown.

There is a class of stone-cutters, men without any anatomical knowledge, who perform lithotomy by the lateral perineal method, plunging a bistoury into the bladder without the use of a staff. I have no statistics for settling the mortality, but suppose it to be about 30 per cent.

Syphilis being for the most part of the secondary form, acquired by the use of the pipe, is confounded with the non-specific disorders which it simulates, and treated as they are.

In the removal of tumors, malignant and non-malignant, there is, as might be expected from their want of anatomical knowledge, no courage nor enterprise, and few operations are ever attempted.

Aneurism is a disease of which they do not understand the pathology, and the cure of which by ligature they would not dare

to attempt, owing to their ignorance of the existence of the arteries as a distinct class of vessels, and their anatomical position and relations.*

Diseases of the bones and deep-seated neuroses are usually treated by extensive applications of the actual cautery. The writer has seen a case of necrosis of the femur which had been treated by double lines of cauterization along the length of the limb, and a circle of fire above and below the knee-joint.

A great prejudice exists against amputation, especially among Moslems, from a fear that the mutilation will be perpetuated in the resurrection-body. Most patients would rather die than suffer this operation. In cases of gangrene they will allow the tedious operation of natural amputation to be consummated rather than submit to the knife of the surgeon.

The treatment of diseases of women exhibits ignorance and charlatanism in an astonishing manner. The most drastic emmenagogues are given fearlessly to promote a free flow of the menses, and thus, as is supposed, to favor fecundity. Innumerable nostrums are administered by the mouth, or injected into the vagina and rectum to contribute to the same end. Every old woman has her infallible method of bringing about this much desired consummation, and the result is seen in chronic leucorrhœa, ulcerations of the os uteri, and general wreck of the health and strength of the unfortunate victims. An American practitioner, for many years a resident of Beirût, informed me that, according to his experience, four-fifths of the native married women in that city were afflicted with ulcerations of the os and leucorrhœa, due to this cause.

Of diseases of children they know nothing definite. Ignorant of all means of physical diagnosis, they cannot determine the nature of internal disorders of infants, or if they think they have identified the disease, they apply the principles of treatment which are in vogue in practice among adults. Thus the use of the lancet on infants of the tenderest age is of daily occurrence. For aphthous spots on the tongue and lips they use the actual cautery in the roof of the mouth and in the fauces. For convulsions, the result of irritation in teething, the actual cautery is applied over the anterior fontanelle. Hardly a head among the natives of Syria is free from a bald spot and depression in the apex of the scalp, the mark of fire applied in infancy.

The usual treatment for the lumbricoid worm is the use of pel-

* Fractures and dislocations are set by goatherds. The limb, in cases of fracture, is bound up from the time of the accident, in bandages stiffened with the albumen of eggs, mixed with flour. Where mortification does not take place, this treatment secures good coaptation and satisfactory results. The practice is borrowed from the treatment of fractures occurring among the goats of their flocks, where, owing to the small amount of fleshy covering over the bones most exposed to fracture, there is far less danger of mortification than in the fleshy legs and arms of men.

lets of soap, taken on an empty stomach, followed by a cathartic. It is eminently successful with children.

Epilepsy, without reference to its pathology, is treated by numerous applications of the actual cautery to the scalp and other parts of the body. The root of the Leontice Centopetalum, an indigenous plant of the country, has been used empirically, with considerable success, in this formidable complaint. Its virtues are now undergoing a test in Bellevue Hospital, in this city.

The periodic fevers, which constitute the most characteristic class of the diseases of Syria, are treated according to the worst ideas of pathology and therapeutics—copious depletion, at every recurring paroxysm, absolute inattention to all proprieties of diet, the disturbance of the feeble, often delirious patient by the ill-advised visits of friends, the non-use of quinine and other tonics, all conspire to give a large mortality to severe cases, and greatly protract the convalescence in those which recover.

In continued fevers, the extremes of Sangrado practice still prevail, with their well-known results.

With a race of practitioners who superadd their medical attainments to the callings of blacksmiths, barbers, goatherds, and ignorant priests and sheikhs, it is not wonderful that Syria presents to the eye of the traveller a melancholy spectacle of the frequency of lameness, deformity, blindness, and emaciation from chronic diseases, which a proper treatment might alleviate or cure.

The superiority of the western science and skill is already asserting itself, and the Syrians have begun to resort to the European and American practitioners in Beirût for the treatment of ailments that have hitherto been abandoned to fate, in the belief that they were quite incurable.

It cannot fail to interest members of this Academy to know that by American enterprise and zeal a Medical College has been inaugurated in Beirût, and that the work of regeneration of medical science among the Arabian race is already in progress.

I bespeak from you, gentlemen, your sympathy and aid in the noble work for humanity and science which it is the aim of that institution to accomplish.

In answer to a question from Dr. Garrish, Dr. Post stated that cataract is not infrequently met with;—when treated, the cases (just as cases of stone) are treated by operators from Syria, and Dr. P. thought that they have reasonable success; they have no idea of delicate instruments; but in regard to cutting for stone, he believed they always cut without a staff, hit or miss; consequently about fifty per cent. die.

Dr. Garrish wished to know if Dr. P. had seen much of operations for strabismus; to which the doctor replied, he had met with cases, but no operations; thought Dr. Van Dyck had often operated, but had not been able to undertake many operations himself.

Dr. Neftel, of Russia, inquired as to the form of the skull of the

natives. Dr. Post, in reply, stated :—There is a peculiarity in the Arabs, that when a child is born its head is pressed at the back, pressed upwards and forwards, and is kept upon a hard pillow, consequently the skull becomes flattened behind, projecting upwards—sort of pyramidal. The same gentleman inquired as to the prevalence of syphilis ; to which the doctor replied :—It is not so common as in this country ; he thought that it is often transmitted from one person to another by means of smoking, it being customary for a guest to smoke from the pipe of the host ; and if there be a syphilitic taint, Dr. P. thinks it is often transmitted in this way.

Dr. Metcalfe wished to know if miasmatic fevers are common there.

Dr. P. stated that they are very prevalent and fatal. They treat them in the most extraordinary manner. Their practice is very disastrous indeed ; they have no idea of sick wants—no hospitals, or asylums for the deaf and dumb. In their religion there seems to be no mercy—different from the Christian religion. They give indigestible food, in the opinion that it is strong. Infants are often fed upon eggs fried in butter, and other articles equally indigestible.

Dr. Bulkley inquired what instrument they have for bleeding.

Dr. Post replied that they have lancets, and bleed without any regard to the arteries. He saw a man bleed an infant right over the brachial artery, but fortunately without any evil result.

Stated Meeting, June 4, 1868. Dr. A C. Post, President, in the Chair.

Dr. George Edward Post, of Beirut, Syria, was elected a Corresponding Fellow.

Dr. A. S. Church read a paper on "Albuminuria during Pregnancy," after which Dr. Griscom read a supplementary paper on the same subject.

ALBUMINURIA IN PREGNANCY.

The term albuminuria is usually applied to a class of diseases arising from a disturbance in the functions of the kidneys, in which albumen is found in the urine, and anasarca is a conspicuous symptom. In a pathological point of view, the name is not well selected, for it represents only a symptom, and diseases that clinically bear a close resemblance, both in their general symptoms and results, may differ widely in their pathology, and by this nomenclature be classed under the same general head. Thus, in Bright's disease we have to a greater or less extent a disorganization of the glandular structure of the kidneys. In that form of albuminuria following scarlatina, typhoid fever, and cholera, we have a congested condition of the kidneys caused by

a blocking up of the tubuli uriniferi by excrementitious matter and fibrinous clots, and which may sooner or later end in destruction of the delicate structures, and become genuine Bright's disease. While in the albuminuria of pregnancy, we have a passive congestion of the kidneys from venous obstruction. But in all of these different conditions, the functions of the kidneys for the time are interfered with. Albumen escapes into the urine, urea is retained in the blood, and almost precisely the same symptoms follow in one condition as in the other.

That form of albuminuria which occurs in pregnancy, will be the subject of this paper.

That condition of the system which is accompanied with albuminous urine and its consequences, may develop itself at any period of pregnancy, but does not usually attract attention until about the sixth or seventh month. When occurring earlier than the third month, there is strong reason for suspecting that disease of the kidneys existed before pregnancy occurred. The frequency of its occurrence is a matter upon which it is almost impossible to get accurate data. In private practice it is wholly out of the question. Those who practise largely among the poor, where it is less embarrassing to obtain specimens of urine for examination, find that at least one out of three of their patients are primiparæ; multiparæ, and many easy deliveries going into the hands of midwives. The same objection exists in regard to statistics obtained at hospitals and lying-in asylums, a large proportion being primiparæ, giving a result that is not quite satisfactory. Blot found among the patients of a lying-in hospital, that one in five had albumen in the urine. Abeille found in private practice one in ten. Elliot one in fifty-six. Experiments, too, will show different results upon the same patient at different stages of pregnancy; for instance, the urine of a patient which at the fifth month shows no trace of albumen may become decidedly albuminous at the eighth month. The probability is that not more than one pregnant woman in 75 or 80, exhibits albumen enough in her urine to create disturbance, for the presence of a small amount of albumen unaccompanied with the symptoms of uræmic poison is of but little consequence.

The symptoms of albuminuria as usually first observed by the attendant, are almost constant headache, nausea, retching and vomiting coming on during the latter months of gestation, pain in the hypogastrium, impaired appetite, loss of strength, restlessness and inability to sleep, sometimes constant drowsiness, œdema, sometimes general, sometimes observed in the most different but single parts of the body, and being of much more consequence as a symptom when observed in the face and upper extremities than in other localities. One or two or several of these symptoms may exist for days or weeks, without attracting marked attention, when the pain in the head suddenly increases to such an extent as to be almost unbearable, restlessness and wakefulness increase, and finally eclampsia follows. Sometimes the attack is

much more sudden than I have described, the patient retiring at night after having apparently been well during the day, with a severe pain in the head, vomiting, great restlessness, and is taken with a convulsion before morning. In attacks coming on so suddenly there is usually a large increase of albumen in the urine, taking place coincidentally with the attack. Patients that have suffered with this disease for several weeks, generally present a decidedly anæmic appearance, the skin being pale and waxy, œdema of the face being much more marked in the morning than at any other period of the day. Albuminuria will sometimes exist during a large portion of the period of pregnancy, and none of the symptoms of uræmic poison be developed. Again, certain patients will show an idiosyncrasy for a certain symptom or train of symptoms. For instance a patient of mine has, during three of her pregnancies accompanied with albuminuria, exhibited no marked symptom but amaurosis, each time the sight being gradually and completely restored after delivery. Another will be troubled with mental illusions of a most annoying character.

Nearly all the causes which produce Bright's disease may be enumerated as causes of this form of albuminuria, but two conditions of things more particularly present themselves which, together or separately, are capable of explaining its occurrence—passive congestion of the kidneys from pressure upon the renal veins, and an altered condition of the blood peculiar to the pregnant condition. As the womb enlarges, it rises into the abdomen between the fourth and fifth months of pregnancy, and the resistance that it meets with from the abdominal walls and viscera, causes it to press upon the inferior cava and renal veins, causing a passive congestion of the kidneys, which results in an appearance of albumen in the urine. That this is an efficient cause is evident from the fact that primiparæ, in whom the abdominal walls offer a greater resistance than multiparæ, are more frequently the subjects of albuminuria.

That obstruction in the renal veins is a sufficient cause for the exhibition of albumen in the urine, is shown by the experiments of Meyer, Robinson, and others, in ligaturing these veins. They always found albumen in the urine after these experiments, and when only one side was ligatured, albumen was found only in the urine escaping from that side.

The altered condition of the blood, no doubt, depends in a great degree upon the disturbance in the functions of the kidneys, by which it is rendered porous in quality, and contains a large amount of excrementitious matter, the former contributing largely to the œdema, and the latter furnishing the elements for the formation of uræmic poison.

A disturbance in the functions of the digestive organs and the skin, the former causing disordered nutrition, the latter a retention in the system of much effete matter that usually escapes with the perspiration, each undoubtedly contributes to this result. The

escape of albumen into the urine, the loss of appetite, constipated bowels, and other evidences of disordered nutrition, and the almost constant dryness of the skin, afford abundant evidence that not one alone, but all of these disturbances, combine to bring about this result.

If the theory of pressure upon the renal veins by a gravid uterus is admitted to be a cause of albuminuria, then in a similar way, plural pregnancy, and an excessive quantity of amniotic fluid, may both be considered causes of this condition. A constipated state of the bowels, whereby a large accumulation of fæcal matter in the intestines keeps up a constant pressure upon the renal veins, is undoubtedly a frequent cause of the trouble, and every one knows how rapidly the most alarming symptoms will disappear, and the albumen also, by free purgation, when the trouble arises from this cause.

Acute congestion of the kidneys from cold, is a frequent cause of albuminuria, and when arising from this cause, the attack is usually sudden, commencing with a chill, severe pain in the lumbar and præcordial regions, almost complete suppression of urine for a time, followed with bloody urine for several days.

A case occurred a few weeks ago in my practice, a description of which will give a fair illustration of the mode of attack when arising from this cause. Mrs. M., when about eight months advanced in pregnancy, after unusual exposure, was taken with a chill, severe pain in the lumbar and hypogastric regions, vomiting, and suppression of urine. Up to the time of the attack she had been perfectly well. The subsidence of the pain was followed with a copious flow of bloody urine, which lasted for several days, and when it was sufficiently free from blood to be submitted to the usual test for albumen, an abundance was found. Intense headache, occasional vomiting, and restlessness, continued for several days, but no convulsions. She ceased to feel the motion of the child from the time of the attack, and although she carried it to the full term of gestation, it was stillborn, and evidently had been dead for the last month. The urine was albuminous up to the time of parturition, and for several days after. Another case, arising from the same cause, but differing in many particulars, is of some interest. Mrs. D., in her sixth pregnancy, was delivered by a midwife, and everything progressed favorably until the fourth day, when she arose from her bed and wiped up a pail of water that had been spilled upon the floor by the children. She was soon after taken with a chill, severe pain in the abdomen, vomiting, and suppression of urine. She gradually became comatose, and died on the fourth day of the attack, in profound coma. I learned on inquiry that she had been troubled with headache and some œdema, but not enough to cause any inconvenience. A small quantity of urine was obtained by the catheter, which contained albumen. A thorough post-mortem revealed no evidences of disease sufficient to cause death, except congestion of the kidneys.

I am not aware that either age, station in life, habits, or temperament, exert a very perceptible influence in causing albuminuria; neither have I discovered that a patient is particularly liable to a return of it during succeeding pregnancies; although I can recall several instances where this has been the case. When the disease occurs for the first time after a woman has borne several children, there is reason to fear that it arises from a disorganized kidney; and that in all probability it existed before the last pregnancy occurred. Also, when it does not disappear within a few days after delivery, grave apprehensions arise that we have a case of genuine Bright's disease to deal with.

The pathological condition of the kidneys in a case of recent standing is that of simple hyperæmia, without destruction of the delicate structure of the organ. The renal capillaries become distended by venous obstruction, and their walls easily transmit the serous portion of the blood, the tubuli uriniferi are completely blocked up with fibrinous exudations and blood corpuscles, the choking of the tubes necessarily impedes the depurating functions of the kidneys, and the blood is poisoned with excrementitious matter. When the congestion has existed sufficiently long to cause more or less destruction of the organ, the pathological condition becomes that of Bright's disease in some of its stages, a consideration of which does not properly belong to this subject.

The result of albuminuria upon the pregnant woman, when the disease is gradual and progressive in its character, is to impair the appetite, disorder the organs of nutrition, deteriorate the quality of the blood, both by the excess of urea and of the other detrimental ingredients which the disordered kidneys have failed to eliminate, thereby producing a condition of the system usually called uræmia, the toxæmic effect of which is eclampsia, coma, and often death.

The most formidable effect of uræmia upon the pregnant woman, is puerperal eclampsia. This seldom occurs before the sixth month of gestation, oftener during the process of parturition, and occasionally during the lying-in state. When it is recollected that a large proportion of the cases of puerperal convulsions are from other causes than uræmia, such as epileptical, hysterical, and apoplectic, and no other form except the latter is so fatal to both mother and child, the exhibition of this symptom becomes a matter of the most serious apprehension to the obstetrician, as well as to the friends of the patient. Fortunately for the welfare of our patients, eclampsia does not occur very frequently. I believe that the combined result of observations made by a number of obstetricians upon a large number of patients, fixes the proportion at about one in 485 cases of confinement, and according to Braun, 80 per cent. of all the cases of eclampsia are in primiparæ. It is very likely that this latter estimate is too large, for we must remember that the primipara is much more susceptible to hysterical and emotional influences,

and consequently to convulsions arising from these causes, than the multipara, which would probably lessen the percentage somewhat. The proportion of cases of albuminuria that end in eclampsia, is not easily ascertained, for there is no doubt that its existence is often overlooked, particularly when no striking symptom is present, such as severe headache, vomiting, or œdema. Cazeaux states that about one in four or five cases of albuminuria end in convulsions. Braun gives the proportion to be one in ten. The immediate cause of eclampsia is not yet well understood. Several ingenious theories have been advanced, but no one has been fully accepted. I shall simply allude to two or three of them. The generally received opinion is, that urea is the poisonous ingredient in the blood, and causes convulsions by its irritating action upon the cerebro-spinal centre. Lehman and Frerich, whose opinions Braun has also adopted, think that the cause of the convulsions may be sought for in the ammoniacal contents of the blood, produced by the transformation of urea into carbonate of ammonia. Others consider a reflex action upon the spinal centre, caused by an excitement in the uterus or some distant organ, sufficient to produce the result. I do not propose to discuss the different theories, but simply to express an opinion that true puerperal convulsions are caused by some poisonous in gredient in the blood, urea probably being the principal poison, acting as an irritant upon the cerebro-spinal centre, while such convulsions as are caused by excitement of an organ distant from the nervous centre, such as an irritable uterus, an over-distended bladder, or distention of the intestinal canal, are likely to be of a hysterical character.

In my experience, uræmic convulsions occurring as early as the sixth or seventh month of gestation, have not as a general rule been very violent, often recurring, or long continued, sometimes the patient having only one or two convulsions, followed with a semi-comatose condition lasting for several hours, and with perhaps no return of the convulsions for a week or two, if at all, but usually followed by the death of the fœtus. In fact I do not know that I can recall a single instance when the mother has had a true uræmic convulsion, that has not been followed by the death of the fœtus, unless delivery has been effected in a very short time. Eclampsia occurring at or near the termination of gestation, is pretty sure to be followed by labor within a few hours, the convulsions apparently being the provoking cause. Neither the frequency nor severity of the pains seems to have much influence upon the convulsions. Uræmic convulsions occurring during labor, usually make their appearance without many premonitory signs, except headache, and great restlessness, are quite regular in their recurrence, usually coming on every thirty or forty minutes, generally continue until parturition is completed, and often several hours longer, and the patient in the interval remains in a somnolent or comatose condition. The convulsions

are epileptic in their character, and the danger to the patient is oftener from the congested condition of the brain, or effusion into the arachnoid membrane, than in any other way, although in plenty of instances a post-mortem reveals neither congestion nor effusion, the patient evidently dying from uræmic poison. Convulsions occurring during the lying-in state, do not differ materially, either in their character, mode of appearance, or results, from those occurring in labor. Several of the diseases and accidents of the puerperal condition are so often attendant upon, and so liable to follow albuminuria, that they may very properly be mentioned under this head. I allude to puerperal mania, puerperal peritonitis, and post-partum hæmorrhage.

The effect of uræmia upon the fœtus is often fatal. It may die from different causes: first, from being insufficiently nourished in consequence of a deteriorated condition of the blood; second, from the effects of urea transmitted to it from the mother; third, from interruption of circulation during the convulsions.

When the mother has suffered for a considerable time from uræmic disturbance, it is quite usual for the fœtus to die before the full period of gestation is completed, even though the mother may not have had convulsions; and if it does reach maturity, and is born alive, it is usually feeble, and dies within a few hours or days after birth, sometimes without any violent symptoms, but often in convulsions.

That children sometimes die from convulsions while in the uterus, is quite probable. Cazeaux mentions two cases where the children were born with a contracted state of all the muscles of the limbs immediately after expulsion. I delivered a woman a few weeks ago, by artificial dilatation and the forceps, of a dead child, in which the muscles of the extremities were perfectly rigid. The arms were thrown out from the body at an angle of about 45 degrees, and on being pressed up to the side, would fly back like a bent whalebone. The mother had been in convulsions about twelve hours. Children of uræmic mothers are often stillborn without the mother having suffered either from convulsions or a particularly hard labor, apparently dying only a few minutes before birth, and sometimes with actual pulsation in the umbilical cord, but no effort at respiration. On several occasions in my obstetrical practice I have been surprised at the birth of a well-developed but stillborn child, without any apparent cause for its death, and am well satisfied that an examination of the mother's urine will in some instances clear up the obscurity, by revealing the existence of albuminuria that had not been suspected.

In the treatment of albuminuria, several indications demand our attention. The alleviation of uncomfortable symptoms, such as headache, vomiting, and œdema, the improvement of the general health of the patient, the restoration of the healthy functions of the kidneys as far as possible, and the treatment in eclampsia.

When there is reason to suspect that the condition of the pa-

tient is caused or accompanied by constipated bowels, a brisk purge of pulvis jalapæ compositus is proper, and usually gives great relief. The daily use of this remedy in small doses, when there is a tendency to constipation, is of the greatest benefit both in relieving the headache and œdema. When the patient is of full habit, the attacks of headache very severe, and the quality of the blood not much deteriorated, I would occasionally resort to the old, and now much abused remedy—bloodletting, and particularly if the danger of an attack of eclampsia was imminent. I have seen patients that had suffered the most agonizing headache for two or three days, completely relieved by the loss of twelve or sixteen ounces of blood. I do not believe in the general use of the lancet in this disease, nor in its frequent use, but I think occasions occur when it is proper, and should not be discarded on account of the fashionable prejudice against it. Bromide of potassium, when venesection is unsuitable, given in twenty-grain doses, gives great relief. Diuretics for the purpose of removing the œdema, are of but little use, and often do harm. Diaphoretics, such as the hot bath, and the hot-air bath, are very suitable, and sometimes afford considerable relief. When the symptoms are not so urgent as to require immediate relief, and a few days' time can be had for treatment, I have found the iodide of potassium given in ten or twelve-grain doses four times a day, to lessen the amount of albumen in a few days very decidedly; and although not a specific, in at least five out of six of the cases in which I have used it, the benefit has been very marked. The exhibition of the remedy for about a fortnight generally removes nearly all traces of albumen from the urine, and at the same time is followed with considerable lessening of the œdema. On discontinuing the remedy the albumen soon makes its appearance again, and will continue to do so until the cause (the pregnant condition) is removed. Anæmic patients require iron and other tonics, change of air, and such other treatment as is calculated to improve the general health. In an attack of eclampsia, bloodletting in my hands has not been followed with much benefit. I do not hesitate to bleed in some cases when the patient is plethoric, and when the brain is in considerable danger, but I do not think that it lessens the number or frequency of the fits. It may control their violence.

Chloroform by inhalation is the most valuable remedy that we have; yet, valuable as it is, it may be used so injudiciously as to cause the death of the patient. As the convulsions usually return with considerable regularity, it is advisable to commence the use of the remedy a few minutes before the expected fit, and by continuing its use uninterruptedly, for fifteen or twenty minutes, the period is passed over without a return of the convulsion. Its use should be resumed in time to anticipate another convulsion.

In this way chloroform may be used successfully, without using it constantly. I prefer not to use chloroform while the convulsion lasts, nor until the asphyxia—the result of the convulsion—

is over. Bromide of potassium in full doses, when the patient can swallow, is an excellent adjunct or sequence to chloroform. It quiets the restlessness that usually exists when there is not complete coma, and is much less liable to do harm than opium.

The induction of premature labor as a preventive measure in threatened eclampsia, is a remedy that has numerous advocates. The great danger to both mother and child if eclampsia occurs, seems to be a sufficient justification for so bold a proceeding in certain cases; but when we remember that it is so common to meet with cases of the most alarming threatenings, that complete their period of gestation, and go through the pains, excitement and fatigue of labor without convulsions, and when we reflect upon the great difficulty of artificially dilating the os uteri before the neck is completely obliterated, and bringing on labor at our will, the liability of losing the child, and the danger from inflammation by our artificial means, our duty does not seem so plain. It is very seldom that an artificial premature delivery is attended with safety to the child. If it is not lost during the delivery, it seldom lives many days, and this fact ought to be borne in mind in deciding upon our course of treatment. Whenever it becomes necessary to resort to this treatment, the plan likely to be the most successful, both in point of time and comfort to the patient, is the warm douche, until the rigidity of the os is somewhat overcome, followed with the use of Barnes' dilators. If the process of dilatation is not well borne by the patient, it is better to use an anæsthetic. The danger from convulsions is lessened, dilatation is effected with more ease and rapidity, and the patient is saved unnecessary suffering. When a patient is in convulsions, and they continue, and both mother and child are in danger, I would dilate and deliver as expeditiously as possible, not only using rubber dilators, but my hand, if better means were not at my disposal; and if pains are not strong enough to expel the child, would resort to the forceps, or podalic version, to complete the delivery. When I say I would deliver as expeditiously as possible, I mean without injury to the mother. Twice within the last two years I have seen a rigid os that barely admitted the index finger, dilated by the hand alone, and a child nearly mature, turned and delivered in half an hour. Of course it was a very bloody operation, and the children still-born. The mothers died also. They might have died had the treatment been more gentle, but not by rash and injudicious treatment.

When a patient has had only one or two convulsions, and no immediate threatening of a return of them, and the child is still alive, although there may be no doubt about the uræmic origin of the convulsions, is it advisable to bring on labor artificially as a preventive measure? I think not, for perhaps the uræmic intoxication may have exhausted itself, and there will be time for other and safer treatment. Again, suppose that after two or three convulsions we ascertain that the fœtal heart has ceased to beat,

the coma is passing off, there is no headache, no immediate prospect of a return of the convulsions, and no labor-pains, shall we proceed to bring on labor or wait until nature does it? Shall we not run the risk of doing more harm by our artificial delivery than the dead fœtus does? I think that after the fœtus is dead, and the womb ceases to increase in size, the danger from a repetition of the convulsions is very much lessened; nor am I aware that a dead fœtus retained within the uterus for two or three weeks, is followed with such unpleasant consequences to the mother as to call for interference.

Convulsions from other than uræmic causes never require this kind of treatment, the danger to both mother and child being comparatively small.

After the reading of the papers, Dr. Peaslee remarked that albuminuria is not a disease of itself, and is never to be treated as albuminuria. If it is caused by congestion, or inflammation, they are to be treated and removed. Albuminuria, therefore, is not necessarily of very serious consequence. He examined the urine of three patients pregnant, in the last month, two of whom had albuminuria; the one who had no albumen in the urine at all went three weeks beyond the time of her reckoning.

Dr. Gouv'r M. Smith wished to know of Dr. Peaslee, if the albuminuria in patients is due to a watery condition of the blood, and if so, if the protein compounds would not be appropriate; and if, on the contrary, it is due to organic changes, should they not be prohibited.

Dr. Peaslee stated that when he was speaking of hydræmia, as one of the causes of albumen, he stated that we could do very little, if the cause was in the kidney, except to sustain the patient in a general way, and keep the skin active.

Dr. Hubbard wished to know of Dr. Griscom, if albumen was ever found in the blood in excess, to which Dr. G. replied: It could not get into the kidneys without being in the blood.

Dr. O'Sullivan inquired as to the use of chloroform in puerperal convulsions, stating that he had had a case in which he used chloroform for many hours without success. He then had a consultation and suggested blood-letting; the suggestion being accepted, the patient was bled freely with good effect—had only three convulsions afterwards. He had tried chloroform in other cases and had failed. Thought that bleeding would relieve the distended womb better than anything else.

Dr. Church said: You are aware that I spoke of chloroform in high terms; of course it fails sometimes, so does bleeding and all remedies. Dr. C. had a case within the last month; the patient was kept under the influence of chloroform twelve hours; twice during the time it was neglected to be administered; the first fifteen convulsions occurred in the first six hours, and only two in the rest of the time. In his hands it had been a very serviceable remedy.

Dr. Peaslee said: I use chloroform in two conditions; in convulsions, and rigid os uteri. He knew of no other remedy so reliable. In regard to the power of chloroform to decompose urea in the blood, he would not rely upon that at all; he believed if it acts at all, it acts in diminishing the irritability of the nervous centres.

Stated Meeting, June 18th, 1868. Dr. A. C. Post, President, in the Chair.

Dr. George E. Post, of Beirût, Syria, was inaugurated Corresponding Fellow.

Dr. A. C. Post exhibited a modification of Ferguson's Bristle Probang, for removing foreign bodies from the œsophagus: modified by Dr. Sands. Has been used by Dr. Sayre.

The doctor stated that Tiemann of Chatham street keeps them for sale.

VENTILATION.

Dr. W. F. Thoms read a very elaborate and valuable paper on a new mode of ventilation, illustrated by models and diagrams. In the discussion which followed, Dr. John H. Griscom thought the plan of ventilating referred to in the paper was a very good suggestion.

Dr. Van Kleek wished to know how the air from the tube is to descend, as the author of the paper stated that the temperature of a room might be at the floor 40°, at the middle 75°, and at the ceiling 100°.

Dr. A. C. Post suggested that that difficulty might be overcome by heating the air in the tube; but Dr. Griscom said it could not well be increased to the temperature of the air in the room.

As the paper is likely to go into the hands of people who are not scientific, Dr. Howard thought some explanation in regard to the analysis of the temperature of schools, lecture-rooms, working shops, and other buildings, would be necessary, the author not having stated why the temperature varies so much in the different buildings, and that the air is hotter in the upper part of the room.

The doctor said Dr. Thoms' model was a very pretty one, but he could hardly see the analogy of the effects in a tenement house. And so far as he had been able to understand the diagram, it seemed the system very closely followed that of Mr. Leeds, with one important exception, that of creating a current.

Dr. Hamilton stated that he did not think there is any question in the whole of sanitary science, so much agitated as this one of ventilation, but that certain points of late have been settled; one, for example, that air should be admitted near the floor; and another, that the point of egress also should be near the floor. But he did not understand Dr. Thoms to advocate this.

In Dr. Griscom's judgment, the most appropriate ventilation is

from the upper part of the room, because the carbonic acid gas becomes diffused through the air.

Dr. Hamilton said, gentlemen versed in ventilation say the air should be taken from near the floor, and then you want to carry it to the roof of the building (by a shaft or chimney). If you can only create in this shaft a current of air producing a vacuum in the room, you will have the air diffused. In Dr. Thoms' model the air absolutely falls into the room.

Dr. Post believed it to be a matter of importance that the air should escape from above and below, as certain products (as carbonic acid gas) will accumulate about the floor, as is demonstrated by placing a dog upon the floor in a cavern containing this gas; he is soon destroyed by the carbonic acid gas, while a man will hardly experience its influence. The carbonic acid in cold places accumulates at the bottom, but where it is hot it will ascend.

Dr. Howard said, it might be interesting to see the new mode of ventilation now in use at the church, corner of Fifth Avenue and Nineteenth street, which consisted of a tube, about 10×18 inches, in each corner of the church, in which is placed a gas jet. He went there last Sunday night and observed the temperature at the lower part of the house to be 70°, and near the top, 82°. He believed the trustees thought it better than the old system. It is called "Gouge's" system.

Informal Meeting, July 2d, 1868. Dr. A. C. Post, President, in the Chair.

TALIPES VARUS.

Dr. Quimby, of Jersey City, exhibited a little patient twelve months old, whom he had had under treatment for the past eight months for talipes varus, using his new method (by adhesive straps). Formerly, he had commenced his treatment about two or three weeks after birth; but in this case, an aggravated one (the doctor exhibited a plaster cast of it, taken before commencing the treatment), the child was about four months old when the treatment was commenced, and there was a good deal of resistance, but he thought the result very satisfactory, and now intrusts the case entirely to the mother's care. Dr. Quimby said there is one advantage in this mode of treatment over tenotomy, there is no cutting or excoriation of the skin; if the child is anæmic or weak, this method can be resorted to with impunity, or if the talipes is due to paralysis or partial paralysis. Here the doctor exhibited a specimen of the adhesive plaster he now uses for the purpose—Milhau's, on Broadway; he thinks it is better than Davis's, which he used formerly.

OVARIAN COLLOID.

Dr. Peaslee reported the following case:

I was requested to visit a lady forty years of age, who had

been married twelve years, but had never been pregnant, who had an enlargement of the abdomen; and had been believed by Dr. Studley, her attending physician, and by Dr. T. G. Thomas, to have an ovarian tumor.

She had first noticed some abdominal enlargement two years ago; but most of it had come on during the past nine weeks. Up to that time she had enjoyed good health, but it had failed rapidly the last two months.

I found her in a good state of flesh, and not presenting any cachectic appearance, though she most of the time was confined to her bed. The circumference of the abdomen was forty-one inches, and its development was equal on the two sides. Fluctuation was evident, but indistinct; but it evidently extended from every one point of the enlargement to another. I thought that the diminished fluctuation was due to the thickness of the abdominal walls, and concluded that the case was one of monocystic ovarian tumor. The recent very rapid development of the disease, however, suggested the probability of its malignant character, and I proposed to practise an exploratory tapping before deciding to attempt ovariotomy.

On account of the thickness of the abdominal walls, I first made an incision three-quarters of an inch deep, with a bistoury, which bled very freely for several minutes; when, on thrusting a trocar of large size through the abdominal wall, at the usual point, not a drop of fluid was obtained. I then passed a bent wire through the canula, six inches into the peritoneal cavity, and which could be freely moved in all directions; but which, on removal, was not even moistened in the least.

Leaving the canula in place, I next introduced another trocar, three inches above the umbilicus, and with precisely the same result as before. After, however, introducing the bent wire six or eight times, I at last obtained a particle of gelatinous substance; and thus encouraged, I at last obtained a mass equal to half a drop from the top opening made.

Concluding that I had a case of colloid disease, I advised no further operative interference.

The patient became much prostrated forty-eight hours afterward, and without fever, or any signs of inflammation; and died in two days more.

Post-mortem.—On opening the peritoneal cavity, it was found distended with a peculiar gelatinous mass of great consistence, and a dead white color, weighing twenty pounds, and which, on microscopic examination, proved to be colloid. The pressure of this had completely collapsed the alimentary canal, and accounted far the fact that cathartics had for several days not produced the slightest effect.

Removing the mass, two polycystic ovarian tumors were found, the left being five inches by six inches, and the right about three by four inches. The former contained the same colloid matter found in the peritoneal cavity.

It was a question of importance in the case, whether the operation of tapping hastened the patient's death. But not the slightest inflammation existed in the site of the two punctures. I should add, that the parietal peritoneum was very rough, and presented a peculiar sodden appearance. I have thus far never met with a similar case; and the only one with which I can associate it, is a case reported several years ago by J. F. Jenkins, of Yonkers.

COXALGIA: MORBUS COXARIUS.

Dr. A. C. Post said: About twenty months ago he was called to a case of morbus coxarius, in which he had since taken much interest. The patient was a boy, ten or twelve years old. Suppuration had taken place, and pain was referred to the hip; and the doctor thought some portion of the bone had been discharged. The child was in a very feeble condition, suffering from the long-continued suppuration. Proceeding to disarticulate, the structure of the bone was found to be very much altered by disease; the head of the bone very much absorbed, several points necrosed.

The boy went on favorably for several months, and the parts healed. There was considerable shortening of the limb, and a considerable degree of laxity about the hip; but still it was firm enough to bear some weight. In the meantime, the opposite limb was attacked by the disease, suppurated a long time, and the discharge was very abundant; a number of sinuses formed. The doctor detected rough bone, and about seven or eight weeks ago made a free incision over the head of the bone (which was found to be anchylosed with the pelvis), and divided it; it was divided again below, in the hope that an artificial joint might be formed, with the chain saw, and the piece removed, leaving a difference in the length of the limbs of only three-eighths of an inch. Previous to this operation there was a difference of three or four inches. The doctor was not yet certain whether there is going to be movement, or if there is any false motion; had seen him to-day, and his father lifted him up; was able to put both of his feet upon the floor. He thought that his condition will be very much improved by the result of the operation. This was the only case the doctor knew of, of excision in both hips.

Dr. Post then said, that he meets with a great many cases of vascular tumors in childhood, and is in the habit of tying them off; but recently he met with one of larger size—about as large as a hen's egg—which he thought too large to be treated by ligature. He adopted another mode of treatment, by passing a threaded needle through in different directions, the threads having been wetted with the liquor ferri per-sulphatis. He had treated three or four cases this way, successfully.

Dr. E. R. Peaslee said he had used the citrate of soda successfully in the treatment of diabetes. He had made use of different remedies, the tincture of muriate of iron, nux vomica, etc.

At the end of about four days after the use of the citrate of soda, the sugar diminished, and soon not a trace of sugar was to be found.

STATED MEETING, OCTOBER 1, 1868. DR. A. C. POST, PRESIDENT, IN THE CHAIR.

DR. ROBERTS read a paper, the "History and Physiology of Menstruation."

MENSTRUATION: ITS NATURAL HISTORY AND PHYSIOLOGY, AND ITS RELATIONS TO AMENORRHŒA.

The subject to be discussed this evening is one of great practical importance and intense physiological interest. Much has been done of late to clear up many of its mysteries, although much yet remains to be explained.

One great function of the uterus, if so it may be properly called, is menstruation. The derangements of this function are either due to the predominance of organic lesion, or to a simple alteration of the physiological conditions of the parts. This may be altered in one of three ways: by defect, excess, or deviation. Menstrual troubles alone should be included in the category of functional alterations of the uterus.

In order to understand in what these consist, it is necessary to precede any remarks on either with a study of the *Physiology and Natural History of Menstruation.*

Menstruation consists in a flow of blood, or true hemorrhage, producing itself naturally from the uterine cavity in an intermittent manner, habitually regular and periodical, save with certain exceptions, from the age of puberty, say 12 or 15 years, up to that of 45 or 50.

It is a true function, or act, intimately allied to the reproductive function of the female, and is performed during a varying period, commonly perhaps 30 years, indicating, as a general rule, by its appearance and regularity of return, at once the aptitude of the woman to conceive and the most favorable periods for fecundation, as well as her good general health and local organic integrity.

The normal intervening period between the two consecutive hemorrhages being about one month (Schweiger says 27 days and some hours), the terms "menses," "menstrua," "monthlies," in the Greek, "catamenia," and various other popular expressions are employed to signify the same, with which all are familiar. "Curses," or "turns," are with us, perhaps, the most common. The French call them "les retours," or "règles," and a woman who menstruates well or ill, is said to be "bien ou mal règlée." The Hebrew term was "flowers," still sometimes even yet employed, and the "custom of women." These terms are the phenomenal

expressions of the function of menstruation, or the exterior symptoms which characterize it.

Menstruation as a general rule, when not precocious, occurs at the period of puberty, of which it is the visible and outward sign. Puberty completes, when it occurs, which is marked by certain changes in the female morale and physique, the period of and preparation for, the ripening of the ovum; and menstruation generally attends this period, although they are not necessarily connected. In girls who menstruate, the signs of puberty, increased size of the body and mammæ, hair on the pubes, etc., etc., are perhaps never absent. If no sign of puberty exist, at or after the proper period, malformation of the genital apparatus may reasonably be suspected, and in these cases, also, the signs may exist without the function occurring, as in imperforate vagina and absence of uterus. Some girls, in delicate health, attain to this period without the function; the maturation of the ovum is delayed and menstruation is thereby rendered tardy. In others enjoying good health, it is only a question of time. Suppressed or tardy menstruation may be either a consequence or sign of, or a cause of ill health, in women: least frequently the latter.

To form a just idea of the nature of menstruation we must study—

1st. The manner in which the phenomenon is accomplished each time that it is produced; and

2d. The differences which it presents in its evolution in different women, according to circumstances; and,

3d. The cause to which it is to be ascribed.

The cause of the *periodicity* of menstruation being unknown, we can only look upon it as a law of the female economy, or, as Denman says, "an appertainment to the female human frame, ordained by the Almighty."

It may be divided into three periods: Invasion, existence and decline, or cessation.

First, of its *invasion:*

This is presaged by certain characteristic phenomena. Change of color of the sexual discharges a day or two before the menstrual flow; the utero-vaginal mucus contracts an odor *sui generis* and becomes brownish and stains the linen. Heat and itching at the vulva are often felt; a few blood-globules mix with the mucus and epithelium. The discharge increases, becomes darker, and gradually, in a day or two, assumes all the characters of blood. At other times, it diminishes after some hours and becomes normal, as if all were then over. Then, after a day's interval, a discharge of almost pure blood appears suddenly. Sometimes there are no premonitory signs whatever, the patient finding herself suddenly "taken unwell," at others, for a day or two, malaise, headache, lumbalgia, bearing down, etc., may be felt.

Having reached its highest degree, the hemorrhage lasts (*ex-*

ceptis excipiendis) three to five days; consisting of almost pure blood of arterial character, mixed with uterine or vaginal mucus, swimming with cells of uterine and vaginal epithelium, and even a few globules of pus, in an abundant liquid, which is only the serum of the blood mixed with the genital mucus.

This period coincides probably with that of the tumefaction and distention of the vesicles of Graaf by their own proper fluid.

Cessation.—The flow diminishes, changes from red to brown, red globules diminish and replaced by a mucus first thick, then thinner, and returning to its normal condition. This period probably coincides with that *of the rupture of the Graafian vesicle* and expulsion of the ovum. Please to observe that it this be so, impregnation is possible only at or after the *cessation of the* catamenial flow.

It is now proper to examine into the physiological conditions of the organs concerned in this act, and productive of the hemorrhage. Some days before the occurrence of the catamenia, turgescence of the whole uterus, or its neck, the ovaries, frequently the vagina and vulva, is observable. The parts become swollen and redder, often partaking of the deep venous hue of pregnancy. Their heat is sensibly augmented and their sensibility increased. Everything indicates a state of congestion. The ovary is tumefied, the Graafian vesicle first distended, then broken, the tubes in a state of erection, gorged with sanguinolent mucus, the uterus often doubled in volume, its mucous membrane swollen as in the beginning of pregnancy; in fact, in every case, more or less of a decidua is formed. Thus are the organs put into a condition to fulfil their functions of fecundation and conception. The ovule is to be detached from its bed in the ovary, transported by the tube into and retained in the uterus, depressed to form for itself a nidus or lodgment in the swollen mucous membrane destined to surround itself with it, which is to become, under the stimulus of impregnation, *decidua*, and to unite itself to the ovum, and to separate at a later period from the organ to which it belongs. Locally, heat, tension, itching, and weight are felt by the woman. Constitutional indications of the three great elements of menstruation: fluxion, congestion, and evacuation, by which latter, the fluxionary movement is arrested, and gradually the congestion and nervous erethism are dispersed and dissipated, and finally extinguished.

The evolution of menstruation, although generally uniform, differs in the period of its first establishment, and in the preceding and accompanying phenomena, their duration and abundance.

The facts known or supposed to be known on each of these points are, briefly, as follows:

1. *The age at which the first appearance takes place.*

The medium term between 12 and 18 years is 14½ years. Roberton, in 2169 cases, makes the average 14 years 10 months. Tardier menstruation is more common than precocious, although many authentic cases of the latter are on record. Wall reports a

child who menstruated at 9 months and was afterwards regular; another case at 3 years; Carus, one at 2 years, regular once a month; Cooper, one at 4 years; London Medical and Physiological Journal at 3½; Francis, 4½ years; Whitmore, one which occurred a few days after birth, returning regularly every 3 weeks, till the fourth year, when it died—the child was greatly developed; another at 3 years, regular for several months, the mammæ very large, as at 20 years, nates and labia ditto, much hair, hymen perfect, and vagina large. From 13 to 16 years, however, is the commonest period, much depending on climate, place of residence, whether town or country, habits and mode of life, profession, race, temperament, constitution, and state of health or disease.

In a table of 600 cases of different climates, I find the lowest 8 and the highest 24; medium, 12 years 11 months in temperate climates; 16 years 7 months in cold climates. Cold, as a general rule, retards menstruation. It is earlier in towns than in the country; among the rich rather than the poor; among the well-fed than among the less so; among the stout and plethoric, than in the weak and pallid. All these statements are exceptional.

Roberton, 450 cases: 10 at 11 years, 4 at 20 years; Whitehead, 4000 cases: 9 at 10 years, 970 at 16 years; De Boismont, 2352 cases: 1 at 5 years, 3 at 24 years; Guy, 1500 cases: the greater number at 15 years, next 14 years, then 16 years, earliest 8 and latest 25 years, after 19 years, number small. Dr. Guy goes on to say: in 400 cases the menses *ceased* at from 27 to 57 years, the majority between 40 and 50. The sooner menstruation commences, the longer it lasts. He mentions also 2 cases where menstruation recurred regularly during the whole period of lactation, and one in which it occurred both during pregnancy and lactation.

Precocity of menstruation is sometimes hereditary in families. There is on record the case of a mother and eight daughters, all of whom menstruated at 11 years.

There are modifications concomitant with the onset of menstruation—some pathological, some physiological.

In some healthy women, menstruation occurs suddenly and unexpectedly, without any particular accompanying troubles, and is a source of surprise and alarm. Sometimes the *prodromi* are slight and last only a few hours; in others, symptoms more or less grave may occur, and last some days.

The *general prodromi* are swelling and pain in the breasts, a sensation of fulness and weight in the hypogastric region, slight abdominal tympanitis, lumbar pains, a sero-mucous vaginal discharge, itching, etc.; sometimes aggravated to the point of acute illness, disappearing with the occurrence of the discharge; returning sometimes at subsequent periods, especially if irregular, which they often are in the beginning; then they become regular, and so continue until the period of cessation.

The *interval between the catamenial epochs* is habitually twenty-

five to thirty days, or a mean of twenty-eight days, or a lunar month. Roberton, in 100 cases, found that sixty-one women menstruated every month, twenty-eight every three weeks, one every two weeks, and ten at uncertain intervals. There are great differences. In most women it advances; in fewer, it is retarded. Sometimes after one appearance, two or three months elapse before a return; after one year, the periods become regular.

In some women, although they are not too close or frequent, there are signs of an *intermenstrual* molimen, incomplete as to hemorrhagic flow. In some women, habitually affected with leucorrhœa, the secretion is then notably increased.

In some women menstruation is very abundant, and frequently reproduced, the menses recurring, for example, every twenty days, or else there is an *intermenstrual hemorrhage*—a supernumerary flow, due either to a nervous erethism, to a lymphatico-sanguine temperament, to a plethora of the vascular system, or to a debility more real than apparent. Negrier thinks that in these cases the ovaries act alternately; Tilt calls it remittent menstruation, the result of diseased action. Little is known about it.

Duration.—The menses flow generally for five days—from one to eight—during which the blood flows continuously, more by night than by day, greatest on the third day, and sometimes of irregular duration in successive periods.

The quantity of blood lost varies from 120 to 240 or 300 *grammes* (Courty). It may be only a few drops, or a true hemorrhage, followed sometimes by a leucorrhœic discharge. It is least in stout, active women, greatest in the opposite class in hot climates.

A patient of Brierre de Boismont "had the complaisance" to lend herself to the investigation, and allowed a speculum to remain in her vagina for ten hours and the fluid to be collected. It flowed, I suppose, continuously, and amounted to one ounce. The *analysis* was as follows: in 100 parts—water, 90, fixed matter, 6, consisting of fibrine, albumen, and coloring matter, extractive and fatty matter, salts and mucus. The composition of blood is about the same. Nevertheless, there is this peculiarity about menstrual blood *that, when retained in the uterus*, it is tarry and not coagulated, and by Hunter and many others has been deemed a "secretion."

Menstrual blood was once considered to be a peccant material, and its discharge necessary and salutary. Among the Jews, a menstruating woman was considered as unclean: and the fourteenth chapter of Leviticus contains the laws of her purification for seven days. Everything she touched, or that touched her, was unclean and required to be purified, and cohabitation with her was forbidden while she had her "flowers," Meigs says, under penalty of death. Pliny says that nothing in the world is *more monstrous than menstrual blood.* It turns wine sour, destroys

seeds, hedges, and flowers, tarnishes metals, mirrors, and ivory, destroys bees in their hives, rusts iron, and causes rabies (Chap. 7, B. 28). Such ridiculous ideas are now exploded. It is natural to women, and ever has been.

There is no difference, microscopically or analytically, between blood and menstrual fluid. It is coagulable outside of and within the uterus and vagina; only it is viscid owing to its admixture with uterine mucus in variable proportions.

It is sometimes darker, sometimes paler than natural; sometimes almost serous, owing to certain pathological states. It is said to have an acid reaction. Letheby says that blood from an imperforate hymen is treacly and black, contains no fibrine, its blood-globules are altered, and it coagulates when heated to 212° Fahr.

Certain phenomena precede or accompany the catamenia, of greater or less importance; sometimes null, sometimes a little nervous depression, dizziness, sometimes hot flashes, gastric disturbances, and in some diarrhœa at or during the flow, a blue circle round the eyes, and a feeling of general lassitude, etc.

The *local symptoms* are, notably, those of congestion. Some women are said to be more amorous at these times than at others; animals are so only in the "rutting" season.

Duration.—Menstruation lasts from twenty-eight to thirty years, or even thirty-five; ceasing earliest in those who menstruate precociously. There are individual differences. It lasts longer in women who become regular early, and have had many children. Twenty-eight to thirty years is the mean. In ordinary cases, from six to eight years of a woman's life are spent in menstruating.

Some well-formed women never menstruate. Oldham mentions one, aged forty-eight, married since the age of fifteen; sexual appetite good, and second case twice married.

The age at which the menses cease varies considerably. From forty-five to fifty years for half the women; forty to forty-five for one-fourth; thirty-five to forty for one-eighth; for another eighth fifty to fifty-five years. Some have ceased to menstruate at twenty-one; others go beyond sixty and even to sixty-five or (Richerand) seventy. The *menopause* is latest in cold climates (Roberton, seventy-seven cases: one at thirty-five, one at fifty-seven, two at sixty, and one at seventy).

Change of Life.—The cessation of the catamenia, called among us change of life, is sometimes sudden and unforeseen, or due to some sudden, moral, or physical impression. Sometimes there is simply a progressive diminution in the quantity of blood evacuated. Sometimes the returns are irregular—say every fifteen days—amounting to hemorrhage; sometimes retards of several months, with increased or diminished quantity. Certain nervous or circulatory disturbances occasionally attend the cessation of the menses.

Women are generally very anxious about the cessation of their menses, and apprehend serious dangers from the change of life which they are about to undergo. But these *fears are not justified by statistics.* Generally, it takes place without danger, whether tardy or precocious. Many even enjoy better health than before. Premature *menopause* is often due to violent attacks of disease, or to debilitating circumstances, violent moral emotions, destruction of the ovaria, etc.

Prolongation.—Sometimes the menstruation is prolonged to fifty-seven, or even sixty-five years; and in the Parthenologia of Schurigius, 1790, there is a whole chapter devoted to facts of this character. In one recorded case, menstruation ceased at fifty-two, and recurred at sixty-two; and continued regular until seventy-three. In a second, the menses ceased at fifty-two, recurred at sixty, and *continued till ninety!*

Cases of pregnancy, at advanced ages, are not uncommon. But when discharges of blood recur, after long intervals of amenorrhœa, they probably depend upon other causes than ovulation, and disease may be suspected. Again, after a menopause of six years, menstruation has recurred for a year, followed by pregnancy, and safe delivery; in another case, in a woman of sixty-one, after twelve years of cessation. Errors of diagnosis might, in such cases, easily arise.

Table of return of menses in eighty-two women, after confinement, B. de Boismont: One immediately; two in fifteen days; four in three weeks; nine in one month; thirty-eight in six weeks; seven from five to six weeks; seven in two months; six in three months; two in four months; three from five to six months; two from seven to eight months. Are these not some hemorrhages, and some miscarriages?

Some women, although well formed, *have never menstruated;* although often owing to anatomical defect. Some, again, *only menstruate during pregnancy.* There may, in young girls not menstruated, be ovulation without hemorrhage, as autopsies show.

Marriage, which sometimes induces menstruation, on the other hand often suspends it for several consecutive periods.

Generally, *the menses are suspended during pregnancy and suckling.* As a general rule, the two acts do not go on together. Uterine hemorrhages are not uncommon during the early months of gestation. Most women, who menstruate during lactation, are not good nurses; their milk, at the least, is less abundant.

After confinement, menstruation is suspended for a varying period. The menses rarely reappear before six weeks; often much later; Meigs says, most commonly about seven months *post partum*, and sometimes not at all; in which cases sterility is probable.

Acute diseases sometimes hasten, increase, or suspend menstruation. But all hemorrhages (uterine epistaxes) occurring during

acute diseases, are not menstrual. Many French writers have treated of this subject. The initiatory stages of typhoid fever, and of the exanthemata, and of thoracic and abdominal phlegmasiæ, offer oftenest instances of this kind, which are unimportant as to prognosis, and require no therapeutics, unless the hemorrhage be such as to constitute an important complication.

Supplementary paragraph.—Barnes says, that of one hundred women, thirty-seven menstruated during lactation, seldom retarded beyond twelve months; four menstruated during pregnancy and lactation; of the thirty-seven who menstruated during lactation, twenty-four conceived also. Of the one hundred, fifty-six had conceived during lactation; they were prone to conceive and abort.

The essential cause of Menstruation.—It is only of late years that this, meaning thereby the uterine congestion with which ovulation is accompanied, has received any considerable development, and been investigated with some success (Coste, 1837); and, although still enveloped in much obscurity, is said by an investigator to be one of those physiological discoveries which do honor to our age, and which has done much toward establishing a more real and scientific study of uterine diseases.

It is proved that in women, and in the females of the mammiferæ, in rutting time, the vesicles of De Graaf may break spontaneously without coitus or fecundation; and that the ova, arrived at maturity, may escape naturally from the ovary, pass into the oviduct, descend into the uterus, and be at length expelled from it, if ever they arrive without being decomposed at the vaginal orifice. This, I take to be a synopsis of ovulation; and, if attended with hemorrhage, of menstruation.

The internal phenomena of ovulation in women are identical with those of the "heat" or "rut" in animals. The vesicles of De Graaf arrive of themselves at maturity, even in virgins, and burst spontaneously, and expel the ovum, when arrived at the term of its maturity, attended with a true hemorrhage.

Women are fecundible and nubile only from the time of their first ovulation or menstruation. Among those who, of late years, have contributed to our knowledge on this subject, are, Negrier, in 1831, and Coste more recently. According to these, a Graafian vesicle, whose maturation always coincides with the turgescence of the genital organs, pursues the course of its development during the different phases of menstruation, and, according to circumstances more or less favorable, may burst either at the beginning, end, or at any period of that periodical discharge. The ovarian vesicle has been found broken on the first day, and intact, although distended, on the fifth. At each menstruation, then, in women, one Graafian vesicle, at least, markedly predominates over all others, arrives spontaneously at maturity, and bursts at an undetermined moment in the period, to expel the ovum it contains, although it may remain stationary, or be

wholly reabsorbed, in which case no *corpus luteum* will be found. In reference to this subject, Lee has published six autopsies. In each one, *one or other ovary only* was perforated; but in one both ovaries were large; both Fallopian tubes, red and turgid. The same was true in three of five cases, met with by Negrier. In a case I lately read, on an authority I cannot recall, two perforations were found *in one* ovary, from which, on slender filaments, floated two ova, not yet embraced by the Fallopian tubes. Had each of these been fecundated a dual pregnancy would have resulted from one ovary. These leading facts of ovulation were first noticed by Cruikshank, in 1797.

Periodicity of ovulation, although perhaps the rule, is not immutable; many circumstances may occur to hasten or retard it. Generally, however, the menses are periodically reproduced, and during the period in which they manifest themselves are attended by the signs of menstruation; so that the maturation of the ovum, and oftenest, its rupture, or dehiscence, translates itself outwardly by a flow of the menses.

No doubt can be entertained of the relation between ovulation and menstruation. No woman, or mammifer, menstruates, whose ovaries are removed, atrophied, or non-existent. But there may be *ovulation without hemorrhage*, and *hemorrhage without ovulation;* but that is not menstruation. Such are some of the seeming instances of regularity at advanced periods of life, or even during pregnancy.

Hemorrhage and ovulation, although connected, are distinct and independent of each other; not the hemorrhage, however, dependent on ovulation, which only is true menstruation.

I have already alluded to the difficulties which may arise in diagnosis in such cases. Habit and periodicity may continue; hemorrhagic molimen and consecutive sanguineous flow after ovulation ceases, which is often the case after forty-five years. Uterine hemorrhage, I take, in every case, whether pure or ovular, to depend on the hyperæmic, swollen, turgid, congested, or hypertrophic (or hyperæmic) condition of the uterus. Its superficial capillaries split and tear like those of the Schneiderian mucous membrane in nasal epistaxis. Whence else can come such large discharges of blood? Not, surely, only from the torn envelope of the Graafian vesicle, or mucous membrane of the Fallopian tubes. Congestion is the basis of ovulation and of hemorrhage. It may exist, and even occur periodically, without ovulation, as ovulation without it, and depend upon disease. I should think that such repeated congestion after ovulation had ceased, not relieved by sanguineous excretion, might lay the foundation for the establishment of chronic uterine and ovarian disease at the change of life, and give some color to the popular fears entertained respecting this period.

But if women enjoy good health, who have never menstruated, or during long menopausis—if women can and do conceive with-

out hemorrhage (menstruation)—if a very trifling show be as good as a profuse hemorrhage—if it matter not whether they menstruate early or late, cease prematurely, or continue long—and if some fecund animals have little or no rut, we might naturally ask, what is the use of menstruation, meaning thereby flow of blood, at all? The ovary and uterus between them can perform all the reproductive functions perfectly without it. Does it not, therefore, seem to be an unnecessary annoyance and inconvenience, and source of debility and exhaustion? or, is its excess oftenest and almost necessarily connected with a morbid hyperæmia? while, on the other hand, amenorrhœa, not of organic origin, must depend upon a want of due raptus, or congestion, uterine or ovarian, and may be cured by whatever excites, recalls, or produces this; a consideration of some importance in the treatment of this affection.

Again, we may inquire, why is it that multiple gestations are so infrequent? Inasmuch as one vesicle, *in one ovary*, is ripe and ready to burst during the act of ovulation, why, we may inquire, may this not be true of an ovum in the other; or why not more than one, in one or both ovaries, which is certainly true in some cases? Women have twins, triplets, four children even. Now, in these cases, do all the ova come from one ovary, or some from either; which is most probable. It is evident that more than one ovum may find its way into the uterus and be fecundated, during a single act or epoch of menstruation. Nevertheless, it is true, that in most cases of post-mortem examination during menstruation, one cicatrix, or false corpus luteum, only has been found. These are points requiring further investigation.

Again, how does the ripe ovum get from the surface of the ovary into the Fallopian tube and into the uterus? It bursts on the outside of the ovary, looking toward the peritoneal cavity. Is the erected and congested tube ready to be applied at this moment to the ovary, and only to one ovary? Does it extend its fimbriæ to every part of the ovary, and so enclose the ripe ovum, or to a part of the ovary only? and if so, how does the ovum pass along from the place of opening and protrusion, and get into the infundibuliform extremity of the Fallopian tube? Upon this head, Prof. John C. Dalton (Physiol.) says:

"The egg, when discharged from the ovary, enters the fimbriated extremity of the Fallopian tube. This wide extremity is lined with ciliated epithelium; the movement of the cilia being directed towards the uterus, a converging stream, or vortex, is produced, *by which the egg is necessarily drawn towards the narrow portion of the tube*, and subsequently conducted to the cavity of the uterus." This does not, to my mind, quite solve the difficulty.

Again: "If the egg is arrested at the surface of the ovary, and does not *enter* the tube, and is there fecundated, it gives rise to an *ovarian* pregnancy; if it escape into the abdominal cavity, to *abdominal* pregnancy; cases of both of which are on record. If,

again, in its passage through the tube, it is arrested and fecundated, the pregnancy is *tubal.* If the ovum be not fecundated by sexual intercourse, it passes away with the uterine secretions." The most likely time, then, for fecundation, seems to be *during*, or *immediately after menstruation;* but at what precise moment cannot be told. An ovum may be fecundated in the uterus without menstrual discharge, which is only the external sign and accompaniment of ovulation. All that is necessary is, that the mature ovum meet in some part of its course with properly endowed semen; semen, that is, containing active, living spermatozoids.

In regard to the question as to how an ovum detached at a distance from the opening of the Fallopian tube gets into it, to which I have alluded, Pouchet says that, "in many animals, at least, the open and fringed *pavilion* of the tube *completely* (*or totally*) *envelops the ovary.*" I shall again have occasion to revert to the subject in some supplementary remarks I propose to offer on the subject of fecundation.

Source of Menstrual Blood.—The blood which escapes during the menstrual flux is supplied by the uterine mucous membrane in its whole extent by a kind of capillary hemorrhage like that of hæmoptysis; coagulating like any other blood, when poured out rapidly and in large quantity. In inversio and procidentia uteri, and with the speculum, this is readily seen. The *corpus luteum* of menstruation differs somewhat in appearance, but not in nature, from that of pregnancy, and in time atrophies and disappears. A new one is formed at each menstrual epoch, and several are often seen, in different stages of formation and atrophy, in the ovaria of a regularly menstruating woman.

Parents are justly anxious about their daughters at the approach of menstruation, and especially when it is *en retard.* The age of from fourteen to eighteen years is shown by statistics to be a critical one, the mortality in the female sex being greater then than at a later period. Moreover, while the function of menstruation is not well established, the dangers of puberty persist.

Precocity of menstruation is supposed to favor the after-occurrence of disease, and especially of cancer.

Menstruation predisposes to disease both by its defect and its excess. Sometimes it is an indication, when disordered, of uterine disease—influences and is influenced by it. It offers a troublesome interruption to the successful and progressive treatment and cure of uterine diseases. It is susceptible of numerous and various perturbations from various causes.

The menses either do not appear at the period of life when they may be naturally expected, or are suppressed after a variable duration, or they are excreted with pain and difficulty, or they are scanty, excessive, or they recur too frequently. Hence properly arises the division of the *disorders of the menstrual function* into three great classes: *amenorrhœa*, *dysmenorrhœa*, and *menorrhagia.*

AMENORRHŒA.

To the first of these, which is the proper subject of discussion before the Obstetric Section of the Academy this evening, and to the proper understanding and treatment whereof these preliminary remarks on the natural history and physiology of menstruation appeared to be necessary and appropriate, I propose to devote a few lines.

Amenorrhœa properly means absence of the menses; but it must be remembered that there may be ovulation without menstruation, or external hemorrhage. Hence, women conceive who do not menstruate. Menstruation may occur within the cavity of the uterus and vagina, the exit of the fluid being interrupted by a closed os uteri or septum in the vagina. This is A. emansionis, or retention of the menses, depending on structural or accidental malformation, and belongs to the domain of surgery.

The courses may not appear at the proper and expected period of life, from defect of sexual development, or atrophy, or inertia of the ovaries, with or without disease, and be in time overcome. Such women are generally weak and impuberous. Ovulation may cease after having occurred, from certain general or local causes, without the general health being much or at all impaired, or the ovario-uterine apparatus being diseased, and return after a shorter or longer interval with or without treatment. These are the commonest cases met with in practice, and about which we are oftenest consulted. I knew a lady who, for eighteen months previous to her marriage, in May, 183–, saw nothing, and enjoyed fine and uninterrupted health. She menstruated on the night of her marriage, after the first sexual congress; after one or two monopauses, conceived in the October following, and was delivered in the next July.

Prolonged amenorrhœa is apt to be associated with, or to depend upon, chlorosis or anæmia, of which it is popularly considered the cause. It is, I believe, in reality, a consequence or effect; for, if the anæmia be removed, the menses occur. Hence the use and value of iron, one of the surest of our emmenagogues, in this state of things. It is an accident, or an anomaly, rather than a morbid state, although it may be symptomatic of a morbid state, of phthisis particularly, or a true disease. As a general rule, in pregnancy and during lactation, it is a normal condition.

Symptomatic A. (primary or suppressed) may depend upon acute or chronic metritis, or disease of the ovaries, or occur in the decline of, or convalescence from, acute diseases, and during many chronic affections. Many causes suspend the appearance, or cause the cessation of the catamenia while flowing, or afterwards, for longer or shorter periods. Cold, by its revulsive action; grief, fright, violent moral impressions, joy, anger, bleeding, a sea voyage, a sedentary and secluded life, as in schools and convents, etc., etc., are common causes

of morbid impression on this susceptible genital apparatus. It is very common among emigrant girls coming off the sea; less so, it is said, since steamers have been substituted for sailing vessels, the passage being shorter and exposure less. Some women become plethoric under amenorrhœa; others, as I have said, are anæmic. In many cases, evident signs of a monthly ovulation show themselves without concurring hemorrhage. These cases are easiest of management; it is often only necessary to forestall and assist the menstrual molimen. Vicarious hemorrhages sometimes occur from other parts, of which many curious instances are on record; as if nature were trying thus to supply an absent but necessary evacuation. The uterus, rather than the ovaries, is, in these cases, probably defective.

In extreme or doubtful cases, an examination of the anatomical conditions of the sexual organs would be proper, and even essential. I know a young woman who has no vagina, and probably no uterus nor ovaria, though otherwise well developed, who has never menstruated, and who has married since her defect was revealed to her. And however improbable, or however respectable the social position of the patient may be, the cessation of the catamenia should always suggest to the mind of the physician the possibility of pregnancy. A few words as to the principles to be adopted and plans of treatment to be followed in the treatment of amenorrhœa.

1st. When the health is good, the sexual organs known or presumed to be natural, time only is required, and nothing specially need be done; the woman is simply *en retard.*

2d. If periodical congestion occur, indicating attempted or actual ovulation, encourage and assist it.

4th. If plethora exist, diminish it; if anæmia, restore the normal properties of the natural ingredients of the blood.

3d. If no fluxion, then, occurs after a time, try and determine it.

Under the first of these heads may be enumerated gentle purgation, cups, sinapisms to the upper and inner surface of the thighs, feet, or hypogastrium, or mammæ; leeches to the external surfaces of the labia majora, or breasts. If pain and spasms exist, entire warm or hip baths, injections, narcotics, by the mouth, anus, or skin. If inertia uteri is suspected, try ergot and electrization. Venesection from the arm, in cases of excessive inflammatory congestion, according to circumstances as to quantity and repetition, sometimes promptly succeeds, and at other times requires to be repeated at each monthly period for a time. Of course, this is most necessary in plethoric persons, and in amenorrhœa symptomatic of uterine and peri-uterine inflammations.

Marriage is the most natural stimulant to ovarian and uterine activity, and may, under proper conditions of the general health and local organic integrity, be properly and hopefully recommended.

Some of the internal remedial agents, which have acquired a certain popularity and inspired a certain confidence as emmenagogues, may be enumerated: aloes in pill, variously combined, as picra in tincture; Ashwell's injection, originated, I believe, by Dewees, of 10 to 60 drops of aq. ammoniæ in 40 grammes of milk (Courty), repeated several days running, so as to procure a slight leucorrhœa; pills composed of rue, savin, and ergot—of each 5 centigrammes, aloes, 2 to 5 cents., for each pill. The first day three, the second six, the third nine, three times a day, with pediluvia, etc.; the "*apiol*," active principle of the apium petroselinum, in doses of two capsules of 25 centigs. each, twice a day, night and morning; Dewees' tinct. guaiac, alone or ammoniated; the tincture of water-pepper; the Japanese tea (tsat-sin); electricity; Simpson's dilating stems, or pessaries, introduced into the uterine cavity, some of which, composed of different metals, are supposed to exert a galvanic influence; or a peculiar pump-like apparatus, or cupping-pump, to act on the uterine mucous membrane; the infusion of savin leaves, in which Pereira (Mat. Med.) reposes confidence, etc., are some of those which have the sanction of time and high authority; their very number and variety testifying to the obstinacy and difficult remediability of the affection.

Amenorrhœa depending on a debilitated state of system, may indeed lead to tuberculosis, an idea very generally entertained; but it is oftenest a consequence and not a cause. The special treatment of such conditions is not the special or specific treatment of amenorrhœa, further than that what removes the one may remove the other also. Hygienic measures, residence in the country or at the sea-shore, bodily exercise, salt water or other bathing (cold or tepid), flannel, the use of chalybeate mineral spring waters, best taken at their source, amid the distractions and excitement of gay and fashionable company and its concomitant diversions; horse and carriage exercise, dancing, a European or other tour, etc., when attainable, are all useful measures in the treatment of idiopathic as well as symptomatic amenorrhœa, and should not be neglected; all tending, as Professor Meigs says, "to evolve the life force, and concur in the great end of improving the general health; which being renewed, the menstruation follows, as light the uprising of the morning sun" (on Females).

Addenda, on the subject of menstruation. (Pouchet, of Rouen, *Théorie Pos. d'ovulation spontanée, etc.* Paris, 1847.)

No one has treated of the conjoined subjects of menstruation and ovulation with more research and accuracy than the above named author, some of whose conclusions I subjoin.

1. The ovary emits ova, independently of fecundation (coition), at determined epochs, and in connection with a periodical sur-excitation of the genital organs, characterized by "rut" in animals, and menstruation in woman, and certain other animals, chiefly of the monkey tribe; but even this may be wanting.

2. There is an intermenstrual period in which conception is physically impossible.

3. Ova exist in women, and the whole zoological series, pre-existent to fecundation; to the number, it is said, in the former, of several hundred thousands in each ovary (Tappey).

The ovum, to be impregnated, must be stripped of all its ovarial coverings. It cannot be impregrated within the Graafian vesicle. The ovum must be mature and detached. *This occurs at each menstrual period.* The distended ovule rises to the surface, bursts, emits the ovum, which is received by the tube, and by its contractions and ciliary movements carried into the interior. Coition may hasten, but it never determines the expulsion of an ovum. This is the act called spontaneous ovulation.

4. It is in the *decline* of the catamenial emission, not at the *very moment*, that the vesicles of De Graaf burst and the ovum falls.

5. Menstruation consists in the appearance of a periodic temporary excitement of the genital apparatus of the female; an afflux of blood to it and discharge from it. Also, it is characterized by the swelling and maturation of *one of* the vesicles of De Graaf and the emission of its ovum.

6. Menstrual blood does not differ from arterial blood; only, it is mixed with vaginal mucus. (See plate in atlas, of microscopic appearances of menstrual blood.)

7. Dividing menstruation into three periods, invasion, existence, and cessation, it is quite *at the end* of this period that the expulsion of the ovum takes place. Then there is an *intermenstrual* period, *during which* the ovum passes into the uterus, and, if *it has previously encountered the semen*, stops in its cavity and fecundates; or, is expelled, or otherwise disposed of. This period lasts ten or fifteen days, and ends with the expulsion of a deciduous membrane. *Women only conceive during* this period; not after it. Generally, there is only *one ovum* which escapes, either immediately after the cessation of the menses, or in one to four days after.

Amenorrhœa is common among prostitutes.

Dr. Detmold said: About thirty years ago he published an interesting case in the Philadelphia Journal. He was in consultation with the late Dr. Francis. The woman had retention of the menses, and was sick in consequence; during the time of the retention, she became covered with hair, breasts, arms, &c. When the menses reappeared, the hair dropped off again.

Dr. Peaslee said: The paper just read by Dr. Roberts is a very complete résumé of our present knowledge on the subject of menstruation. I however consider the point well established, that the menstrual fluid poured out by the mucous membrane, both of the cavity of the uterus and of the Fallopian tubes, and not at all from the neck of the uterus, is simply *blood*. Menstruation is therefore merely a hemorrhage and not a secretion. The blood, however, becomes mixed with the alkaline secretion of

the uterine cavity, and the cervix, and the acid mucus of the vagina, and is therefore no longer pure blood when it leaves the vagina. The vaginal acid usually prevents its coagulation, but if the flow be abundant, as in menorrhagia, clots are found as in other hemorrhages.

The case mentioned by Dr. Detmold is one of the many curious instances of modified growth of the hair by changed ovarian activity. Hairs naturally exist on every part of the human body, excepting the palm of the hand, the sole of the foot, and the last joints of the fingers and toes, the inner surface of the prepuce, the glans penis, the upper eyelids, and the lips. But they are so fine on most of the surface of the body of the human female as to escape our observation. In this instance, they were generally over-developed for a time, until the ovarian action again became normal. In many cases of ovarian tumor, the fine hairs on the patient's face grow into quite a decided beard; which disappears after the tumor is removed, and the patient regains her accustomed health.

To the case of secretion of milk by a woman of advanced age, I would add one more remarkable one of secretion, to the extent of even nursing an infant by the male. I think the case is well authenticated, though I do not recall all the particulars from the journal in which it was reported. Cases of very large development of the lacteal glands in the male are, however, not so very rare.

Dr. Anderson stated, that in a case where the ovaries were removed by operation, a slight menstrual discharge took place at the period of menstruation from the cicatrix, and healing up again entirely; the woman doing well at the same time.

Dr. Worster said, about twenty years ago he had a case of inversion of the uterus; attempted the operation several times to reduce it, waiting some eight or ten days to repair damages before another operation; was compelled to desist when menstruation commenced, for about four or five days. The lining membranes looked something like a condition of ecchymosis, puffed up, a quarter or half an inch each way, an inch in diameter. The fluid, a thin watery color, looked like watery blood, but still had not the appearance of blood; this would at last break away; about the time it broke down, there would commence to flow a material still thinner, and continue about four days. This was the only opportunity the doctor ever had of watching menstruation with the eye.

Dr. Garrish stated that the early period of menstruation had not been referred to, as it commences earlier in the Southern than in the colder climates of the North; it was said to occur in the Southern climate as early as the tenth or the eleventh year. He remembered one case in New York, of early menstruation; he attended her and delivered her of a child in the twelfth year.

The doctor had seen cases of ladies having a number of chil-

dren without ever having menstruated; said every practitioner has met with cases of ladies having children without any symptoms of menstruation whatever. He knew one instance of a woman having seven children who had never menstruated. There is a very remarkable case related in the London Journal, of a woman being married twenty years, and delivered recently of a child, in her fifty-third year.

Dr. Post stated, that Dr. Horace Green reported a case of a mother who nursed one or more of her daughter's children, and this grandmother had an uninterrupted flow of milk for six years.

Stated Meeting, October 15, 1868. Dr. A. C. Post, President, in the Chair.

Dr. William B. Neftel read a paper on "Exact Methods of Medical Treatment" (in connection with the use of the thermometer).

Dr. L. A. Sayre moved that a vote of thanks be tendered Dr. Neftel for his highly valuable and interesting paper, which he hardly thought admitted of discussion. The vote was unanimously carried.

Dr. Roberts observed that Aitkin's work holds the thermometer to be very important in diagnosis, and another work (Anstey) recommends that every family should have a thermometer in the house, so as to know, by its use, when to send for a physician in doubtful cases, and more particularly among children. The doctor was not aware whether the thermometer has been used in this city and in the hospitals, or not; had used it himself and is certain that it will become very important.

Dr. Bulkley replied that it has been used in the New York Hospital for two or three years, and that a card can be seen upon which the record of temperature is kept, at the head of the bed of every typhoid fever patient. And others are in the habit of using it in this city.

Dr. Neftel said: It has been used a long time, but he thought, not systematically. He had been in a consultation where it was said, that a physician practising without a thermometer, was like a blind man walking through the streets.

Dr. Weir said the thermometer has been used in hospitals in surgical operations, in the use of ether and chloroform. In amputations, it is noticed that, on section through the bone, the temperature of the body is immediately depressed.

Dr. Roberts stated that Fahrenheit's scale is used in Aitkin's work, and the highest temperature given of the human body was 105° or 106° in scarlatina, etc., although it may go much higher.

Dr. Neftel had a case in this city of typhoid fever, with a temperature of $42\frac{3}{10}$° centigrade, and said that the patient was now recovering, the normal temperature of the human body being 37° centigrade.

Dr. Roberts, as the paper referred to cold affusions, thought

that treatment offered great advantage, and ought to be revived and mentioned its use by Currie, Southwood Smith, in typhus and scarlatina, and others.

Dr. Bulkley said Prof. Nathan Smith used cold water sixty years ago.

PARTIAL EXSECTION OF THE FEMUR ON BOTH SIDES.

Dr. A. C. Post exhibited a boy between ten and eleven years of age, on whom he had performed the partial exsection of the femur on both sides, Dr. Sayre being present. He said: This boy was the subject of morbus coxarius in both thighs. It is now about two years since I excised part of the bone on the left side, which was in a state of necrosis and caries. Suppuration commenced in the other limb, and was very obstinate. This limb being two or three inches longer than the other, about five months ago I operated on the right thigh to bring the limbs into a state of parallelism. I have frequently made deep incisions into the bone. The doctor here exhibited the pieces of bone removed. For two or three months after the last operation, a discharge continued. The hip is very much better; he is able to stand upright, and can move rapidly on his crutches. He has considerable motion of the left hip-joint, but the right is anchylosed with the pelvis.

He is only able to sit down in a very imperfect way; has recently begun to go to school; of late years he has been at home on account of his health. He has only begun to walk within the last fortnight.

Dr. Anderson wished to know if the Secretary (Dr. Janes) could give the Academy any information as to the use of the thermometer in the cattle disease, as he had been engaged in its use.

Dr. Janes replied that Dr. Morris would be able to give more information than himself, and that gentleman was called upon, and replied that his observations were made altogether with the Fahrenheit scale. He found, on examining the cattle, that if the temperature ran as high as 105° it would be almost always fatal. To be sure, we do not always let them die, but slaughter them. None have been known to live after a temperature of 105°, and when it runs to 110° it is positive death. The natural temperature is 99° or 100°.

Dr. Van Kleek believed the highest temperature known in the human subject was 106°.

Dr. Sayre saw a case of sunstroke at Bellevue Hospital, whose temperature was 109°.

Dr. Van Kleek wished to call the attention of the medical profession to the meat preserved by Prof. Gamgee. He had tasted the various kinds of meat, but it did not taste of the ingredients by which it is preserved.

Dr. Roberts stated that Charles Dickens, in the Household Words, speaks of a similar process in use extensively in Australia.

Stated Meeting, November 5, 1868. Dr. A. C. Post, President, in the Chair.

Dr. R. C. Stiles remarked: The demonstration which you have kindly invited me, Mr. President, to make to the Academy this evening, is of importance on account of the light which it throws upon a hitherto doubtful point in the anatomy of the liver. It often happens that an accidental observation reveals a truth which has eluded long and patient research. In this instance, investigations undertaken for protecting our markets from the introduction of dangerous food, have had, as an incidental result, a revelation of remarkable clearness of the mode of origin of the bile-ducts within the lobules of the liver.

As long as the office of the liver was believed to consist only in the secretion of bile, it was natural to see in its structure that of other secreting organs, viz., *cul-de-sacs*, or tubes filled with secreting cells and communicating with the duct of the gland. When, however, Bernard discovered the glycogeine function of the liver, and demonstrated the important influence it exerted upon the constitution of the blood, this hypothesis no longer satisfied the demands of physiology. The most recent works on histology and physiology confess that great obscurity still prevails on the question of the mode of origin of the bile-ducts. Kolliker admits no structures within the acini but liver-cells and capillaries. Leydig, of Tubingen, whose work on comparative histology is among the most recent, follows Beale in the admission of tubes filled with hepatic cells. Milne Edwards, after giving the history of discovery on this point, and citing Kiernan, Retzius, Müller, Beale, Leidy, and others, confesses that the difficulties attendant on this study are so great that they will long remain undecided. All these difficulties have faded away before the teachings of comparative pathological anatomy.

One of the characteristics of the Texas disease in cattle is the abundant secretion of bile. It fills the intestines, distends the gall-bladder to its utmost capacity, and, what is most important in this relation, fills the hepatic duct with its finest branches and the network of tubes in which it is seen to originate. The color of this bile is of a bright orange, and its consistence such that the reticulum of bile-ducts is revealed with a certainty, beauty, and permanence which leave nothing to desire. The difficulty of artificial demonstration is seen to arise from the delicacy of the reticulum which renders injection impracticable, and their demonstration when uninjected impossible.

Such is their delicacy and fragility, that, in separating the liver-cells, each cell carries with it its share of reticulum. They are simple tubes of basement membrane, without nuclear or cellular

contents, and hence invisible when empty. Under these circumstances no wonder that they have eluded observation. They are about one ten-thousandth of an inch in diameter, are cylindrical, do not vary in size within the lobule, and form a network by their inter-communication, containing in its meshes the liver-cells. In thin sections, this network presents the greatest regularity; a hepatic cell filling each mesh, which is of a hexagonal or polyhedral form, corresponding to the shape and size of the cell. The reticulum empties into the inter-lobular bile-ducts by branches which scarcely exceed in size the tubes within the lobule. No capillaries intervene between the reticulum and the hepatic cells: the two structures are in immediate contact and adherence, and hence their relation to the capillaries presents no difficulty or complication.

The bile in the Texas disease of cattle owes the intensity of its color to excess of hæmatoidine, and its consistence to coagulable matter from the blood, such as fills the bile in the gall-bladder with orange-colored flocculi; hence, no more favorable conditions could have existed for this study than the repletion of the bile-ducts and their ultimate radicals with such an injecting material.

It has heretofore been easy to imagine the correct structure of the latter, but the Texas disease furnishes the only demonstration yet offered, as far as I am aware, and removes the subject from the domain of hypothesis.

Dr. Thos. R. Pooley was unanimously elected a Resident Fellow.

Dr. Isaac E. Taylor read a paper on "Procidentia Uteri, consequent on the so-called Hypertrophic Elongation of the Cervix Uteri."

Stated Meeting, November 19, 1868. Dr. A. C. Post, President, in the Chair.

Dr. A. D. Rockwell read a paper on "The Treatment of Paralysis by Electricity, with description of some new Galvanic Apparatus."

On the Treatment of Paralysis, with an Explanation of a New Galvanic Apparatus.

Mr. President and Gentlemen:—The chief aim of the paper which I am to present to you to-night, will be to call your attention to the treatment of paralysis, by the scientific use of the galvanic and faradaic currents. But in the brief time that is allowed on this occasion, it will be manifestly impossible for me to enter into anything like a full or complete detail of the operations that I shall describe, or to answer even a tithe of the various queries that will very naturally arise, concerning a system of treatment at once so complicated and unfamiliar. I shall therefore

not attempt to be exhaustive, but shall confine myself to general principles—illustrated by special cases—content if I only succeed in making clear to you what I regard as the true scientific basis for the employment of electricity in the very frequent and distressing affection that we call paralysis. And in order to accomplish this important and special object, it will be necessary to waive all mere theorizing concerning pathology, and to avoid all discussion of words and names, to omit all elaborate descriptions of apparatus, and to confine myself to that which I regard as thoroughly established and of direct practical importance. The name paralysis (derived from παρα and λυω, to loosen), of itself means nothing, and conveys but little idea of the nature of the affection it is designed to express. It sprang up like most of the names—especially of our nervous diseases—in dark and ignorant times, when men knew so little of pathology or physiology, that they were obliged to designate all mysterious affections by some predominant symptom.

The attitude of the profession toward paralysis has, thus far, been that of neglect and discouragement.

The paralytic has been suffered to endure his affliction as though it were a special judgment from Heaven, against which to invoke the aid of science would be sacrilege. The reasons for this discouragement and neglect on the part of the profession and the people, are threefold.

In the first place, there have been very few who have attempted to make any differential diagnosis in the various forms of paralysis. We have forgotten that paralysis is merely a name, a symptom, or an effect of many diverse pathological conditions.

Cases of paralysis dependent on myelitis, congestion of the spinal cord, meningitis, effusions on the brain, and those caused by reflex action, or by nervous exhaustion, have been grouped together, and have all received the same prognosis and the same treatment. The tendency has been to content ourselves with the vague diagnosis—paralysis—which is really no diagnosis at all, and conveys no idea whatever of the indications for treatment.

Secondly—We have been in the habit of deferring the treatment of paralysis until too late. The custom has been to do nothing until the eleventh hour, and then oftentimes to do just the wrong thing. The palsied patient does not usually suffer the great pain that is connected with many other affections, and therefore is more easily tempted to put off all remedial measures until the muscles have become so atrophied, and the general system so exhausted, that recovery is impossible.

In the third place, our discouragement in the treatment of paralysis is largely due to our want of skill, adaptation and perseverance in the application and administration of remedies.

We have left the details of our rubbing, or passive movements, or electrization, to hireling attendants or careless servants, who know nothing of any subject, and least of all of the science or prac-

tice of medicine. We have prescribed batteries, as we have prescribed boluses, to be used at bedtime, and with even less supervision of the practical details.

These three reasons are, I think, sufficient to account for the present unsatisfactory state of the therapeutics of paralysis. There are two very strong arguments why the profession at the present time should give more direct and special attention to the treatment of this affection.

1st. It is certainly increasing in frequency. We think that all our older practitioners agree that hemiplegia, paraplegia, and what has been termed infantile paralysis, are much more frequent now than they were 30 years ago. The same is probably true of all phases of nervous diseases.

2d. With our present means and appliances for treatment, paralysis is much more relievable and curable than formerly.

The principles on which the recent and more successful methods of treatment of paralysis are based are, 1st, internal remedies directed to the removal of the cause; and 2nd, external applications designed to counteract the effects, to improve the nutrition of the palsied muscles, and to give tone to the benumbed nerves. Accordingly we give strychnine in paralysis dependent on anæmia or exhaustion of the nervous centres; ergot, belladonna and iodide of potassium in the opposite conditions of congestion and inflammation.

We give bromide of potassium to calm the irritated nerve-centres and reduce the volume of blood in the brain, and phosphorus as chemical food for the brain.

In order to fulfil the second indication, the prevention of muscular atrophy and the restoration of nutrition in the affected parts, we employ rubbing, passive movements, and electrization. The latter remedy meets both of these indications better than any other that we know of. It is a tonic to the nervous system, and like all other tonics, exercises a calming, soothing, and quieting influence on the brain and spinal cord.

Like other tonics, also, it increases the appetite, quickens the circulation, and makes the sleep more profound and refreshing. It is by virtue of these tonic effects that it is so markedly efficacious in paralysis. Electricity, in some form or other, has been used for paralysis for more than a century.

Authenticated cases of perfect cures have been reported, that have resulted from a stroke of lightning. In its earlier days, the voltaic pile worked wonders in this department, but the current it afforded was unreliable and inconstant, and therefore it caused more failures than successes. Until the discovery of the faradaic current by Faraday, a convenient apparatus for electro-therapeutical purposes was impossible to be obtained; and the most trying annoyances that we have experienced in all our investigations in the department of electro-therapeutics, have been our repeated failures to obtain a convenient and satisfactory galvanic apparatus.

This beautiful combination which I now present to the Academy, was manufactured for us by Messrs. Chester & Co. It was not completed until after considerable labor and investigation, and many annoying details that can only be appreciated by those who have attempted similar experiments. The apparatus may be composed of any number of elements that may be desired. Seventy-five or one hundred, or at most one hundred and fifty cells, will be sufficient for nearly all the ordinary purposes of electro-therapeutics. The cells are each three inches long and are inserted in a wooden block. Each glass is provided with a zinc cover, from which a projection runs down into the glass, thus forming a zinc element. The other element is carbon, carefully connected with platinum, and well insulated from the zinc cover. Between this cover and the glass top, a piece of soft rubber is interposed, and the packing is made completely *air-tight* and water-tight by the pressure of two rubber sponges that pull the cover firmly down.

Connection between the cells is readily made by means of spiral springs. The battery is charged by filling the glasses half-full of water, adding some bisulphate of mercury.

A small bit of cloth is interposed between the plates so as to retain moisture. To use the battery we invert it, thus allowing the fluid to flow over the plates and moisten the cloth. It should be kept inverted but for a short time, and then it should be restored to its natural position. While the apparatus is inverted, and for some time afterward, the current of electricity may be felt by taking hold of the electrodes. The strength of the current and the length of time it can be felt after the immersion, will depend on the number of cells of which the apparatus is composed. In an apparatus composed of 100 cells, a powerful current has been felt for many hours after inversion. In any apparatus all the cells may be used, or only a part of them, according as a strong or weak current is desired.

The first question we are to ask ourselves, when one is presented to be treated by electrization for the relief of paralysis, should be, will this method of treatment do harm? It has been commonly supposed, and so taught by some of the European writers on electro-therapeutics, that the faradaic current is harmful in hemiplegia following cerebral effusion, until many months have elapsed from the date of the attack.

When extensive laceration of cerebral tissue has taken place, it is obvious that the paralysis will, in all probability, remain permanent, and that electricity will be of but little value. But when there has been *merely* an effusion of blood, the symptoms are caused, more by a clot which has formed and separates and compresses the cerebral substance, without producing great destruction of brain tissue. In such a case the health of the patient may be approximately if not completely restored. As a rule, however, we should not wait six months, or even six weeks,

before resorting to electrization, but begin the treatment almost immediately, and direct it especially to the paralyzed motor nerves and muscles. It is rendered probable by the researches of M. Bonnefin, a pupil of Brown-Sèquard, that a very feeble current of electricity may be passed through the nervous centres, but experience has not led us to suppose that such a procedure would beneficially affect atrophy of the cerebral substance, or hemorrhage into the brain. On the contrary, Duchenne has recorded some cases where such injudicious applications have hastened a second apoplectic attack. It is needless for me to say that cases of hemiplegia caused by effusion do not get well so long as the clot remains to press upon the brain tissue.

In these cases nature is the great restorer, and it is only in a very humble way that our services can be of value. The absorption of the offending clot is a painfully slow process. A long time may elapse before there is the slightest indication of improvement, and months, and even years, frequently pass before nature has so completed her work that the paralyzed extremities are of any considerable service. While the process of absorption is going on, the muscles of the affected limbs, from disuse and the want of the usual brain stimulus, suffer from slow but certain atrophy and degeneration, and when the great nervous centre is in a great measure restored to its normal condition the extremities may have been irreparably injured. The electric currents undoubtedly prevent muscular atrophy and fatty degeneration of tissue, and if used promptly, persistently, and effectively, after cerebral effusions, a considerable experience teaches that we would see fewer permanently crippled conditions as sequels to paralysis. As illustrative of the above principle, I will briefly mention a case that is still under observation. The patient is a stout, plethoric man, somewhat past the prime of life. For years it had been his custom to drink five or six times of brandy daily, although he rarely became intoxicated.

During the early part of the present year, while stepping from a Third avenue car, he became unconscious and fell to the ground, and, although he arose by his own unaided efforts, and walked several blocks to his own house, he remembered nothing of the act. His reason returned several hours subsequently, when his right side was completely paralyzed. He remained perfectly helpless for several weeks, when a certain degree of strength gradually returned, so that, by the aid of a cane, he was able to walk short distances. He first observed some decrease in the size of the limb about six weeks after the attack, and when he was sent for treatment by electrization, the thigh had decreased in circumference one inch. There was considerable diminution of electro-muscular contractility in the affected leg, besides a marked decrease of the normal temperature. The faradaic current alone was used, and in six weeks' time, after receiving twenty applications, the limb had increased in circumference half an inch, the

muscular contractions were normal, and but little difference could be noticed between the temperature of the two limbs. In the treatment of this case it was not supposed that we should be able to remove the cause.

The process of absorption goes on so slowly, that the clot still remains, and it will be many months, if ever, before he regains his previous strength and vigor. The atrophy of the muscles was, however, at once arrested. The normal electro-muscular contractility was restored, the temperature of the limb was increased several degrees, and its nutrition generally was much improved.

The efficacy of the galvanic current in paralysis resulting from spinal congestion, and its superiority over the faradaic, were illustrated in a most decided manner in a case that is now under the treatment of Dr. Beard. The history of the case was that general debility had produced spinal exhaustion, and spinal exhaustion had extended to congestion, which had resulted in paralysis of both the upper and lower limbs. When first seen with Dr. Alexander, of Staten Island, she could not move her lower limbs at all, and had very little power over her arms and hands. The interossei were beginning to atrophy. The faradaic current had been used with slight benefit to the upper limbs, but with none whatever to the lower extremities. Five or six applications of the galvanic current, from Chester's air-tight galvanic battery of sixty cells, produced immediate results. She could raise her feet and bring her arms to her head. After six weeks of treatment the atrophied muscles are returning to their normal condition. She is beginning to feed herself, which she had not attempted to do for several months before. She can sit for an hour in a nearly natural position, and with assistance can stand for a moment in an upright position. She is still under treatment, and every week witnesses progress.

But there are paralytic conditions in which electrization is not only useless, but may result in positive injury to the patient. The contra-indications for its use were very marked in the case of a woman who consulted us eighteen months since. In the morning, while dressing, she was prostrated instantly, and for several hours remained unconscious.

Her intelligence returned only partially after the lapse of twenty-four hours, but the paralysis of the right side was complete. Four weeks subsequently, when some slight power of motion had returned, an application of the faradaic current was made to the affected parts.

The diseased limbs were remarkably susceptible to the influence of electrization. A current of very ordinary intensity, and sufficient to produce contractions on the healthy side, would instantly cause the muscles of the paralyzed limbs to become violently convulsed.

An application of a few moments only was invariably followed

for the time being by a complete loss of the little power regained by the paralyzed muscles.

After the experiment had been tried a number of times, it was evident that the patient had lost rather than gained strength, and the treatment was discontinued. According to the experiments and researches of Dr. Todd, the marked exaltation of the nervous force in this case doubtless depended upon an irritative lesion of the brain. Dr. Althaus and others relate cases similar to the above, caused by repeated attacks of apoplexy, by a tumor pressing upon the brain, and in some instances—as in the case just related—by a single apoplectic seizure. When, therefore, in a case of hemiplegia, the electro-muscular contractility of the paralyzed limbs is greatly increased, we may conclude that the patient is suffering from an irritative lesion of the cerebral tissue, and electrization in any form is contra-indicated. Another question that arises in the treatment of paralysis by electrization, concerns the kind of current to be used, whether the faradaic or galvanic.

No. 42, Vol. 2d of the N. Y. *Medical Record* contains an article on Reflex and Organic Paralysis, by Dr. Geo. M. Beard and myself, in which is detailed a case illustrating so decidedly the varying effects of the two currents, that I beg leave to refer to it. Miss P——, a stout, vigorous young lady of fifteen, was sent to us by Dr. F. Elliot, to be treated for paralysis of the 7th pair on the left side, caused by exposure to a draught of air two months previous. The paralytic symptoms came on suddenly and in full force immediately after the exposure, and at first she experienced great difficulty in speaking. She gradually became so accustomed to the abnormal condition of her lips and face that she was able to converse almost as clearly as before, but the improvement in the condition of the paralyzed muscles was very slow, and at the time she came to us the affection appeared to be almost stationary. At the time she received the first application at our hands she presented most of the usual symptoms of paralysis of the 7th pair. Her mouth was drawn over toward the healthy side so violently as to produce considerable deformity when she laughed or conversed, and even when she smiled. When she attempted to frown the left brow remained as smooth as that of a child.

Her left eye rolled up, and when she attempted to close it the lids would not approach nearer than ¼ of an inch to each other. A powerful faradaic current localized in the affected muscles produced very feeble or imperfect contractions; while on the sound side a very mild current, applied with the hand, produced active contractions of all the principal muscles.

The patient was so well in all other respects that we decided to use only partial or localized electrization over all the muscles on the left side of the face. Two vigorous applications made in this way—one electrode being placed firmly below the ear, and the other passed over the ramifications of the 7th pair, as well as over the individual muscles—did not seem to increase, to any appreciable

extent, the electro-muscular contractility, and accordingly we resolved to adopt an entirely different method of warfare. The next time we localized the galvanic stream through the left side of the face, and with most charming results. Contractions of the paralyzed muscles were at once produced that were as vigorous as those caused by the faradaic current on the healthy side. The patient began at once to improve, and after ten visits, distributed over a period of six weeks, she was dismissed as approximately cured. There still remained some deficiency of action of the muscles concerned in frowning and in winking, but the expression of her face, both in repose and in conversation, was normal. The very little that remains nature ought to accomplish unaided.

The interesting points in this case are as follows:

First—The galvanic current produced contractions and wrought a cure, when the induced or faradaic utterly failed. This important fact with regard to the galvanic current, viz.: its power of causing contractions in paralyzed muscles that do not appear to be affected by the faradaic, was first pointed out by the late Prof. Remak, of Berlin, whose original and able researches in electro-therapeutics* really made him the father of a distinct school of electricians in Germany, as Duchenne has long been in France. Though the ideas advanced by Remak were at first scouted and despised by the profession, as indeed has ever been the case with all original suggestions in science, and although his own impetuosity and extravagance hurled him into generalizations that could not have stood the test of investigation, yet his leading idea in regard to the superiority of the galvanic over the faradaic current, in some forms of paralysis, is now as fully established as any fact of science among those who are practically familiar with electro-therapeutics. These observations of Remak have recently been confirmed in a very striking manner by Prof. Zemssin,† of Berlin, by Schulz, Meyer,‡ Baierlacher, Neumann, and Hammond.

Second—The paralyzed muscles were at first brought to contraction by a galvanic stream of small quantity, but of considerable intensity, that had no *effect whatever on the muscles of the healthy side.* As the patient improved, however, it became necessary to use a stronger galvanic stream in order to produce the contractions. Toward the close of the treatment the muscles of the paralyzed side began to respond to the faradaic current. These apparent inconsistencies had also been previously observed by a number of the German electricians already mentioned. The late P. Victor Bazire§ has recorded an experience almost precisely similar.

It does not follow from these facts that the relative value of

* Ueber Method. Electrisirung Gelähmter Muskelin. Berlin, 1865.

† Die Electricität in der Medicin. Berlin, 1866.

‡ Die Electricität in ihrer Anwendung auf Pract. Med. Berlin, 1861.

§ Notes to Translation of Trousseau's Lectures on Clinical Medicine. Part II p. 331.

the two currents is entirely undecided. On the other hand, it may well be questioned whether we do not know nearly as much of the indications of their use and the rationale of the operation of their currents as of any of the internal remedies that are styled specifics. In general electrization, for the purpose of producing a constitutional tonic influence; in partial electrization, with a view to calmative or absorbent effects, and for most cases of localized electrization, we use the faradaic current.

On the other hand, for those exceptional cases of paralysis where the faradaic current will not produce contractions, and when it is desired to affect the retina, we use the galvanic.

We have now under treatment three cases of infantile paralysis, two of which are evidently reflex in character, caused respectively by exposure to cold during the irritative process of teething, and by persistent digestive disorder. In both cases the paralysis was complete. No intensity of the faradaic current which we felt justified in applying to the affected arms produced the slightest effect, but when a galvanic current of moderate power was made use of, the muscles of the paralyzed responded almost as readily as those of the healthy side. The improvement in the condition of the little patient, whose paralyzed limbs resulted from the combined causes of cold and teething, has been very satisfactory. He has regained full power over the hand and forearm, but is still unable to raise the arm readily from the side. Notwithstanding the approximation to a perfect cure, the faradaic current will as yet produce only feeble contractions, while under the influence of the galvanic current the electro-muscular contractility is vigorous.

In the other case the improvement is far less rapid.

There is not the slightest evidence of reflex movement in the diseased arm, and the faradaic current is entirely inoperative in its effects. The galvanic current will, however, produce some considerable contraction of the muscles, and it is evident that whatever favorable results are to be obtained by electrization must come from the persevering use of this form of electricity.

The third case of infantile paralysis to which I referred, occurred in the person of an infant aged fourteen months.

The left leg was entirely powerless, and although the muscles responded with impaired vigor to the galvanic stimulus, the faradaic current produced no contractions whatever. The paralysis had existed eight weeks, but in this case the galvanic current produced results quite unexpected, and I think unusual, for in ten days the child was approximately cured.

A marked feature in the effect of electrization with the faradaic current on very young children, is the difficulty with which muscular contractions are produced. Not infrequently we find it impossible to cause the muscles to respond in the slightest degree to the stimulus of an induced current of sufficient intensity to excite powerfully the electro-muscular contractility of the

limbs of an adult person. This phenomenon is due, probably, in part, to the great quantity of adipose tissue which surrounds and covers the muscular substance of infants, as well as to the insusceptibility of the undeveloped muscular and nervous tissue to the influence of the electric current.

The leading ideas that I have sought to advance in this essay to-night may be summed up in the following propositions:

1st. Paralysis is not a disease, per se, but is merely a symptom of some disturbance of the central or peripheric nervous system. Our treatment of paralysis, whatever system we adopt, should have reference not alone to the affected parts, but also to the general condition of which it is the result, or with which it is associated.

2d. Paralysis—throwing aside the toxic, traumatic, and local varieties—is pre-eminently a symptom of exhaustion or weakness of the nervous system. Therefore it is increasingly frequent under the pressure, and haste, and nervous strife of our high civilization. It occurs most frequently among the cultivated and the intellectual. Therefore, also, we should naturally expect it is most liable to attack those constitutions that exhibit a tendency to nervous disorders, and at the period of life when the nervous system is most susceptible to disease. Accordingly we find that paralysis, though it may and does attack all ages, is peculiarly a malady of the two extremes of life—infancy and old age.

3d. The two cardinal principles of treatment of paralysis are, in the first place, to administer remedies directed to the removal of the cause, that is, the condition of the system, or the local injury of which the paralysis is a symptom; and in the second place to restore the tone and nutrition of the affected parts. The first condition is met by the administration of strychnine (internally or hypodermically) when the central nervous system is anæmic; in the use of iodide of potassium, ergot, belladonna, and bromide of potass., when the spine or brain is congested or inflamed, and by the use of general tonics, such as bark, iron, phosphorus, arsenic, and oxide of zinc.

The second condition is met by rubbing, shampooing, &c.

Electrization, general or localized, with the varying use of the galvanic or faradaic currents, by the consent of nearly all our modern neurologists, fulfils these indications better than any other method of treatment. It benefits paralysis by virtue of its tonic effects on the nervous muscular system. This is indeed the only scientific basis for the employment of electrization, not only in paralysis, but in every other form of nervous disorder.

General electrization, like the other tonics—strychnine, bark, iron, sunlight, &c.—is primarily stimulating, but, like these other tonics also, its permanent effects are to strengthen, to calm, to soothe, to sharpen the appetite, to increase the digestive capacity, to lengthen and sweeten repose, to restore the languid circulation, to improve nutrition, and consequently to cause increase in size and weight.

It is by virtue of these constitutional tonic properties that electrization benefits paralysis.

4th. In the treatment of paralysis by electrization, the distinction between the effects of the galvanic and faradaic currents *is of radical and indispensable importance.* An intense, penetrating galvanic current, from 50 or 100 cells of Chester's air-tight battery, will overcome far greater resistance than the faradaic; will produce muscular contractions in paralyzed limbs where the faradaic is powerless; and will also more directly and positively affect the great sympathetic.

A neglect of these vital distinctions is one of the many causes of our failures in the use of electricity for paralysis.

The magneto-electric machines, in which the electricity is generated by turning a crank, are inconvenient and unreliable, and, especially when unscientifically used, have done the cause of electro-therapeutics almost irreparable injury.

5th. In the treatment of paralysis by electricity, we should first of all make a differential diagnosis, and then studiously adapt our treatment to each individual case. Fresh recent attacks of a local character are sometimes very quickly relieved and cured by the faradaic current alone, but long-standing and severe cases, that constitute the majority, demand the varying and persevering use of both the galvanic and faradaic currents for weeks and months.

We feel the necessity of ever keeping in mind the expressive words of the great Trousseau: "Chronic diseases demand chronic treatment."

6th. The prognosis of paralysis, taking the cases as they run, without reference to their pathology, is far more favorable, at least when treated on the principles here indicated, than is commonly supposed. None need be injured by the treatment here described, while many cases are decidedly ameliorated, and a respectable minority are permanently cured. During the past two years we have treated by electrization, general and local, and by the varying use of the faradaic and galvanic currents, 70 cases of paralysis, resulting from a variety of causes, and manifesting itself in different parts of the body. It may be interesting to sum up briefly, and in a general way, the results of the treatment. Of these 70 cases, 16 were supposed to result from cerebral effusion, causing hemiplegia; of this number 11 suffered from paralysis of the right, and 6 of the left side.

Three of the cases were approximately and two completely cured; three were very markedly benefited; three discontinued treatment after a few visits; in three cases only slight amelioration was evident, while two failed to receive any benefit whatever.

Four cases of complete facial paralysis, two of which involved the right side, one the left, and one both sides of the face.

With the exception of the case involving the 7th pair on either

side, and which probably resulted from organic change, following syphilis, they were all quickly and completely cured. Anæsthesia occurred 20 times. In nine cases the cure was complete; nine of the patients were approximately cured, while two discontinued treatment before any result could be seen.

Four cases of complete paraplegia, three of which received no benefit whatever, while one was approximately cured after a protracted course of treatment. One of the patients died while under treatment. The myelitis from which she suffered gradually extended upward, and paralyzed the vital organs.

Eight cases of infantile paralysis, probably of a reflex character. Four of these patients were completely and rapidly cured, and two were approximately cured, while two received no benefit whatever.

We have treated 12 cases of paralysis, each of which involved but one limb of the body. One of these was approximately, and five completely cured; four derived marked benefit, while two discontinued treatment before any result was noticed.

Four cases of palsy agitans, but in one case only have we succeeded as yet in obtaining even a slight amelioration of the symptoms. Writer's cramp occurred twice. One patient recovered considerable power in the affected hand, while the other experienced no relief worthy of mention.

This aggregate result, though perhaps not brilliant, is certainly most encouraging, and will, I think, compare not unfavorably with the record of treatment of any other severe type of chronic disorder.

The old and familiar Roman maxim, "Obsta principiis"—"Stop the beginning"—is of pre-eminent importance in the treatment of paralysis. If we could at once and forever discard the error that electrization is merely an irritant and a stimulant, and rise to a clear and full appreciation of its tonic and soothing powers; if we could remember to begin our treatment of paralysis in its earlier stages, before weeks and months of neglect have exhausted the general tone of the patient, and shrivelled his palsied muscles, then I am sure that a new history and a new era would open for one of the most common and most distressing maladies of our time.

Dr. Neftel, at the request of the President, remarked:

I wish to ask Dr. Rockwell, how large is the quantity of electricity that can be produced by his new galvanic apparatus.

Dr. Rockwell replied: The quantity is not so great as can be produced by some others. It is the intensity, not the quantity, that is required.

Dr. Neftel: In exact sciences, like physics, we expect positive results, and the question how large a quantity of electricity can be produced by a current means, how many degrees is the needle of the multiplicator declined by the current? or what is the amount of its electrolytic effect, that is, how much water can

it decompose in a certain space of time? There is a mathematical law which dominates the whole theory and practice of galvanism. This law of Ohm is very simple, but its application is so vast that no apparatus ought to be constructed for certain purposes without being in accordance with it. The law of Ohm can be expressed by the following formula, from which all the other modifications are easily derived: The quantity of galvanism produced by a current is in direct relation to the electro-motive force, and in inverse ratio to the resistance. If we consider that the human body offers an enormous resistance to the electric current, and that it conducts fifty million times worse than copper, we can readily understand that a large quantity of a current must be brought into play to produce any physiological effect. In accordance with the law of Ohm, we are able to modify the galvanic arrangement in such a manner as to call forth either calorific or electrolytic and physiological effects. At present, we possess but two excellent apparatuses for the galvanic current—Stöhrer's, and especially Remak's with Siemen's elements; all the others are very imperfect or almost worthless. I insist upon this point, Mr. President, because practitioners who are not acquainted with the laws of electricity may get such apparatuses and apply them to their patients without obtaining the desired effect. This can bring into discredit the whole method, which, by itself, is of the highest importance.

As to paralysis, I am sorry Dr. Rockwell did not speak about the diagnosis of the different kinds of paralysis. Already, in 1862, Ritter discovered the remarkable fact that the ascendant current increases the muscular power, whereas the descendent diminishes it; and according to the laws of Marianini, the descendent current increases the pain, whereas the ascendant increases the muscular activity. This law is extremely important for the successful treatment, not only of neuralgias, but also of paralysis.

The use of voltaic alternatives too has not been mentioned, although their application to the treatment of paralysis is invaluable.

Prolapsus uteri. Dr. E. R. Peaslee: This subject of prolapsus uteri is one of the most important in the whole of uterine pathology; it is a subject which every one of us is called to see, and with which we have to do, more or less.

I am very sorry that Dr. Taylor could not do himself justice with his paper, which was attended with so much labor.

Perhaps there is not a single subject in uterine pathology which has undergone more change in the last ten years than this one.

Twenty or twenty-five years ago, when I commenced practice, all supposed that in cases of prolapsus uteri, the organ descended into the vagina. When it became external, of course the whole uterus came down, and everybody understood that to be prolapsus uteri.

In 1858, when Huguier presented the subject to the French Academy, he stated that, as a general rule, those cases regarded as being due to descent were not such; that they occur almost never. The fact is, the body of the uterus remains in the natural position, while the supra-vaginal position becomes elongated, completely everting the whole vagina. Eight years ago (1860) he published his work on that subject. Attention, however, was directed to that class of cases in this country; but it was very soon found by most practitioners, on the contrary, that while that condition was patent, in a great majority of cases we have genuine prolapsus. Yet I remember, only about a year since, one of the best uterine pathologists in this city wished to know whether prolapsus uteri thus described does ever exist.

During my connection with the Demilt Dispensary, more than fifty cases applied for treatment. Of all I saw in the six or seven years (something like a hundred), only three cases were of Huguier's disease; the rest were true prolapsus; and in my private practice I have very seldom found a case of Huguier's prolapsus. I have met with elongation of the neck of the uterus—I have seen all those forms; but they are very rare.

Now there are three forms confounded, and each requires distinct treatment. First, true prolapsus uteri, where the organ descends in the vagina, carrying the roof of the vagina with it, more or less. Second, elongation and hypertrophy of the supra-vaginal portion of the neck. Third, hypertrophy of vaginal and infra-vaginal portion of the neck. The diagnosis is entirely different, and the treatment is entirely different; and in our practice we must distinguish each from the other.

Now, in every instance of true prolapsus which has continued for a considerable length of time, there is hypertrophy of the whole uterus, and there must be hyperæmia if it has existed very long, in all cases; but Huguier calls attention more especially to the supra-vaginal portion being elongated; the infra-vaginal portion is hypertrophied too. Both hypertrophy of the neck and body may exist separately or together. On the other hand, the infra-vaginal portion is not involved.

Now let us inquire what this hypertrophy is. We generally get the idea that hypertrophy means an increase of all the elements entering into the structure; that is not hypertrophy. Hypertrophy merely implies that the connective tissue of the uterus is increased in quantity.

Understanding what hypertrophy is in this condition, whether it be hypertrophy of the supra-vaginal portion or infra-vaginal portion, or of the whole organ, the most important point of all is, perhaps, to distinguish this condition. The cavity of the uterus may not be increased at all. Now, there is doubtless many a case where there is merely elongation of the neck.

The neck is sometimes very much hypertrophied, very much enlarged, and mistaken for prolapsus. The touch would distin-

guish at once that it was a long neck; but the question is, is it merely a long neck? The neck is sometimes found five inches long; seldom more than three and a half inches.

The uterine probe has got to be used very commonly; but I think the sound answers the purpose of the probe, and many others besides. In my own opinion, I never need the probe unless I have a case in which the os tincæ is very narrow.

I have seen cases where hypertrophy accompanied prolapsus; the uterus was returned and held in place for many months, the hypertrophy entirely disappeared, the neck coming to its natural size. Generally, however, one portion will diminish more rapidly than the other; when the anterior portion being three and a half inches, the under lip would be only two inches; so that I do not see that it is always necessary to amputate the neck.

Huguier recommends amputation. It may be remedied by removing a conical mass, so that what is left of the neck will be like a funnel. Then hypertrophy exists in cases where the whole body is forced down. In that one condition it would be justifiable.

In regard to the treatment of prolapsus, I prefer the use of some kind of support, which will hold the uterus in place, constricting the vagina, to surgical operation.

There is one class of cases which occur in consequence of the rupture of the perinæum; then the thing to do is to apply sutures to the perinæum. In regard to the use of instruments, there are many cases where you can give perfect relief to women with the extremest prolapsus.

In the cases treated in the Dispensary, only three were operated upon; the rest were all treated by supports, and there was not a single instance in which we were not able to afford the necessary relief.

Dr. Kammerer: The paper of Dr. Taylor restrains itself to that form of complete prolapsus which consists in what is called elongation of the supra-vaginal portion.

As regards this elongation, this tumor, on close examination, contains not only a portion of the cervix, but it contains also a portion of the bladder and rectum.

As to the distinction whether in a case of procidentia the vaginal portion or the supra-vaginal portion be increased in size, I confess I have not paid great attention; but it seems to me that it is more difficult to appreciate than Dr. Peaslee seems to suppose.

In the course of the last ten years I have measured every uterine cavity in cases of prolapsus or procidentia, and can positively say that none measured less than four inches, and the majority measured from five or five and a half to six inches.

As regards the operation proposed by Dr. Taylor, I should be very happy to have him state his views. I think he alluded in his paper to a peculiar treatment—treatment of his own.

In response to these remarks of Dr. K., after thanking him

and Dr. Peaslee for having called up the subject of his paper for discussion, Dr. Taylor, at some length, proceeded to again relate his views as to the true nature of uterine prolapsus and procidentia, in reference to its pathology, diagnosis, and surgical treatment; taking issue with the two former gentlemen and M. Huguier on some points, and describing the operation or operations which he should prefer in certain circumstances. He criticised the operation of M. H., of removing a conical portion from the cervix high up, as being hazardous on the score of hemorrhage, preferring either the circular amputation of the cervix, or the removal of a triangular part from each side, and uniting the cut edges with metallic sutures. These operations will be described at length, he said, in the essay which will be published in the Bellevue Hospital Report for 1869, to which the reader is referred.

At the Stated Meeting of the New York Academy of Medicine, held December 3, 1868.

Dr. Gouverneur M. Smith read the following paper on

"VEGETABLE ASSIMILATION."

Mr. President and Fellows of the New York Academy of Medicine:—Intimately associated with that department of medical science which treats of the therapeutical properties of plants, is the science which relates to the structure, functions, and classification of vegetables.

The general laws which govern vegetable life are very thoroughly understood, but it is not usual to examine vegetable nutrition and assimilation so closely as to clearly perceive the method by which plants convert their inorganic food into organic matter; or, in other words, to plainly demonstrate the manner of elaborating the individual proximate principles.

To this branch of the subject attention is at present invited.

Bio-chemical changes are with difficulty observed, and with difficulty proved; occasionally, knowing only the resultants, it becomes necessary to infer what previous organic metamorphoses must have occurred.

Not so with *inorganic* chemistry. The chemist can generally account for the various phenomena which occur when substances spontaneously or artificially react upon one another. Mutual interchanges of elements take place, governed by simple chemical affinities, not being influenced by any vital principle, as occurs in animals and vegetables. The chemist can readily *formularize* such reactions, and often trace each element from its first association to its new affiliation or its distinct isolation. The formula representing the reaction, when metallic zinc is placed in dilute sulphuric acid, can be easily recalled; and the same method of exemplification may be employed to exhibit the chemical changes which take place when many other substances, either organic or

inorganic, artificially react upon one another. While it can hardly be possible to delineate so accurately the changes which take place in organic bodies, it nevertheless may be practicable to approximately illustrate by formulæ the metamorphoses which occur in vegetables, while elaborating their various principles.

Before attempting to do so, it seems desirable, as preliminary to the subject, to take a brief glance at the nature and composition of vegetable substances, and review the general and well-known laws which govern vegetable growth.

It is here proper to remark, that it is foreign to the subject to consider the inorganic constituents of plants. The inorganic salts found in vegetables, though essential, are derived, *per se*, from the mineral masses of the soil, and undergo comparatively few and simple changes after being absorbed.

It is a well-recognized fact that, on analyzing a plant of any class, order, or genus, it is found to be composed of *proximate principles*, e. g., cellulose, starch, sugar, gum, fibrin, albumen, oils, acids, alkaloids, etc.; some of these, as cellulose, starch, and albumen, being universally distributed over the vegetable kingdom; others, as the acids and organic bases, being confined to one genus or to a few genera.

With the exception, perhaps, of the alkaloids, distinct uses have been ascribed to each of these principles in the vegetables themselves, while, when taken by animals, some are nutritive, some medicinal.

We are also aware that, on analyzing these proximate principles, they are found composed of *ultimate inorganic elements;* of the sixty-two elements recognized, but six entering into their composition, viz., carbon, hydrogen, oxygen, nitrogen, sulphur, and phosphorus, these elements being associated for the most part either in binary, ternary, or quaternary combinations.

With the exception of the epiphytes, which draw their nourishment entirely from the air, and of certain other low forms of vegetation, plants, we know, derive these elements both from the atmosphere and soil. Carbon, hydrogen, oxygen, and nitrogen occur both in the air and earth, while sulphur and phosphorus are found in the latter in the form of the sulphates and phosphates.

It is unnecessary to dwell at length upon the sources of vegetable food. The interchange of elements between the animal, vegetable, and mineral kingdoms illustrates the "Balance of Nature," as set forth by Boussingault.

The poet has endeavored partially to exhibit this doctrine when he says:—

> "The man, decaying, gives a food to plants,
> And each to him in turn a living influence grants;
> Thus wheeling round a circle of vitality
> While time shall last, unending as eternity."

The late Dr. Joseph Mather Smith, in writing on the subject of public hygiene, and in calling attention to the relationship between

the animal and vegetable kingdoms, has said: "If this order of nature, so beneficently contrived, and so happily elucidated by Dumas, Boussingault, Liebig, and Fownes, be disturbed, or rather, if but a comparatively small part of the organic matter evolved from the lungs and skin be retained, and accumulated in the stagnant air of human habitations, or become attached to garments, bedding, furniture, etc., it may be metamorphosed into an agent destructive to life; and, consequently, so much be withdrawn from the store of the materials designed for the nutriment and growth of plants. In thus adverting to the mutual physical dependencies existing between man and the lowest orders of organic nature, we arrive at the conviction that it rests with the people or rulers of a city to decide the question, whether the matters transpired from their bodies shall be dispersed over agricultural districts, and in their circle of mutations be again measurably returned to them as wholesome food, or be retained in their dwellings and transformed into a principle productive of disease." *

Our modern philosophy demonstrates a species of transmigration which is material and which is not adverse to Divine teaching, and differs from that fanciful immaterial transmigration believed and taught by Pythagoras.

> "With ceaseless change the restless atoms pass
> From life to life, a transmigrating mass;
> Hence, the same atoms which to-day compose
> The poisonous henbane, or the fragrant rose,
> May, with to-morrow's sun, new forms compile,
> Frown in the Hero—in the Beauty smile."

With this brief glance at these established facts, we will turn to the more intricate subject of vegetable assimilation.

It is a fixed physiological law, first set forth by Priestley and Sennebier, that plants when in activity have the power of absorbing carbonic acid and giving off oxygen. This function is accomplished under the influence of heat and light. According to Professor Lehmann, "it is not only direct solar light, but also ordinary refracted, that produces this phenomenon, which depends not upon the heating or the chemical rays of the spectrum, but mainly upon its yellow and green rays." Morren, in France, and Daubeny, in England, first discovered that it was yellow light which directs the digestion of plants. These gentlemen, however, drew their conclusions from experiments made with light passing through yellow-colored media; it was reserved for Prof. John W. Draper,† of this city, to establish the fact, by employing the solar spectrum itself. With leaves immersed in spring or carbonic acid water, and placed in the different rays of the spectrum, he observed that beneath the yellow beam oxygen was freely disengaged, and the

* Trans. of the Am. Med. Association, 1850. Vol. 3, p. 245.

† Chemistry of Plants. By John W. Draper, M.D. Harper & Bros., N. Y. page 63.

freshly plucked leaves continued their process of elaborating chlorophyl, gum and sugar, fibrin, and albumen. It has been further shown that "the green parts of the plant alone possess the faculty of exhaling oxygen after absorbing carbonic acid." According to the experiments of Boussingault, "the quantity of oxygen that is exhaled corresponds very nearly with the amount of carbonic acid which has been absorbed."

At times, less oxygen is liberated than is contained in the carbonic acid, showing that a part of the oxygen is retained by the plant, while at others more may be given off, for reasons which will hereafter be given.

In attempting to illustrate by formulæ the process of assimilation, we must start with the proposition that water is the basis of nearly every organic principle, or, to speak more accurately, the *hydrogen of the water*, water being viewed as the protoxide of hydrogen, or, in other words, regarded as any other oxide, as, e. g., potassa, soda, etc.

The hydrogen of the water may be deoxidized or oxidized and combined with other elements, and thus be the foundation of most of the proximate principles. The more important bodies contain this element, and those which do not may be formed by the union of unappropriated elements, or by the secondary transformations which constantly occur in the vegetable organism.

Plants can readily obtain water, and can as readily resolve it into its elements. Several proximate principles contain hydrogen and oxygen in the proportion found in water, e. g., sugar $C_{24}H_{22}O_{22}$; other principles contain less oxygen than is required to form water, as quinine $C_{40}H_{24}O_4N_2$, in which case the hydrogen of the water is deoxidized; and lastly, still other proximate principles contain more oxygen than is necessary to form water with the hydrogen, as citric acid $C_{12}H_5O_{11}$, in which case the hydrogen must be oxidized.

It may be assumed that plants possess *two methods* of elaborating their proximate principles: first, the DIRECT, when a vegetable prepares principles immediately from its food; second, the INDIRECT, when it converts principles directly formed into other bodies by processes of oxidation, deoxidation, carbonization, and nitrogenization.

With all these phyto-chemical processes, or with the causes which give rise to them, we are not as yet perfectly familiar, but that such transformations do take place has been conclusively shown by Liebig, Mulder, Draper, and others; indeed, some of them can be artificially imitated.

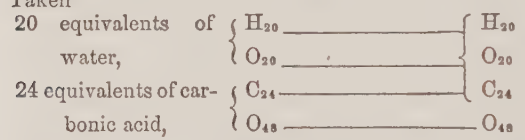
We propose, at this time, to illustrate by a few formulæ the *direct* method only, and we believe that this manner of exemplification will accord with the teachings of Prof. Asa Gray of Cambridge.

Plants, in elaborating proximate principles which contain hydrogen and oxygen in the proportion to form water, have merely to appropriate the necessary amount of carbon from the carbonic

acid absorbed, and unite it with the water. For example, in assimilating dextrine, starch, or cellulose, which are isomeric ($C_{24}H_{20}O_{20}$), they have simply to set apart twenty equivalents of water and unite with it twenty-four equivalents of carbon, giving back to the air the oxygen which they had absorbed with the carbon as carbonic acid gas. These principles, though isomeric, vary in their physical properties, and plants may primarily elaborate either one of them, or, more probably, the soluble dextrine, and then convert one into the other.

The method of assimilating them may be formularized as follows:—

DEXTRINE, STARCH, CELLULOSE (Isomeric), $C_{24}H_{20}O_{20}$.

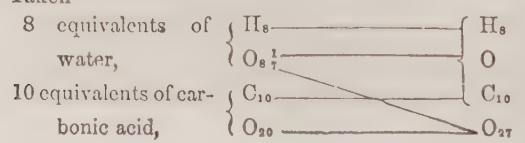


It is thus seen that, in preparing these substances, plants give back free to the air the entire amount of oxygen which they absorbed, combined with the carbon as carbonic acid gas. The chemist cannot unite twenty-four equivalents of carbon with twenty equivalents of water to form dextrine or starch, but we know of the occurrence of such a combination in plants. Vegetable physiology teaches that the elements after being absorbed have more liberal capacities of affiliation than they are observed to possess in any artificial laboratory.

Another class of proximate principles contains less oxygen than in proportion to form water with the hydrogen, and again another which contains no oxygen. In preparing these principles, not only all the oxygen which has been absorbed with the carbonic acid is evolved, but also a large proportion, and in some instances all, of the oxygen which had been combined with hydrogen as water in the plant.

We will cite, as an example of the first, *camphor*, $C_{10}H_8O$. This oil, as is seen from its formula, does not contain, by seven equivalents, sufficient of oxygen to form water with its hydrogen. In order to assimilate it, the plant must appropriate eight atoms of water to procure the eight equivalents of hydrogen, then appropriate ten atoms of carbon from the carbonic acid absorbed, giving off to the air all the oxygen connected with the acid and seven equivalents from the water. This may be illustrated in the following manner:

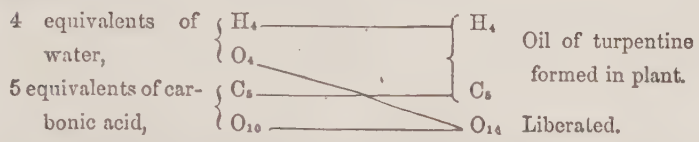
CAMPHOR, $C_{10}H_8O$.



By this diagram it is shown that, in forming this essential oil, the Laurus Camphora enriches the air by twenty-seven equivalents of pure oxygen.

In examining the process of assimilating a proximate principle containing no oxygen, we may cite as an example, oil of turpentine, C_5H_4. Four equivalents of water are appropriated to obtain the required hydrogen, and five equivalents of carbonic acid to obtain the requisite amount of carbon, liberating all the oxygen previously held in the acid and in the water. To illustrate:—

Oil of Turpentine, C_5H_4.



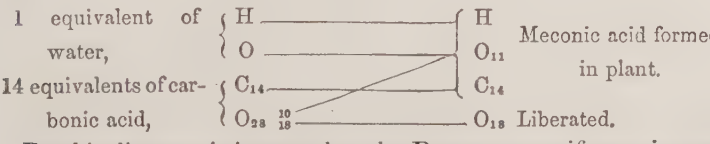
The pines and firs therefore give to the atmosphere fourteen equivalents of uncombined oxygen in elaborating this volatile oil.

Still another class of principles contain, for the most part, more oxygen than in proportion to form water with the hydrogen; we refer to the organic acids, e. g., tartaric acid, $C_8H_4O_{10}$, citric acid, $C_{12}H_5O_{11}$, malic acid, $C_8H_4O_8$. As exceptions to this rule we may mention oxalic acid, which contains no hydrogen (C_2O_3), and kinic acid, which, like starch and sugar, contains hydrogen and oxygen in the proportion to form water ($C_{14}H_{10}O_{10}$).

In elaborating most of the acids, plants have to abstract from the air a small amount of oxygen to oxidize the hydrogen; but though this is the case, they do not appropriate all the oxygen which they absorb with the carbonic acid, and hence, even in these instances, enrich the air by restoring it with pure oxygen.

As typical of this class of principles, we may example meconic acid, $C_{14}HO_{11}$. The plant sets apart one equivalent of water to procure the hydrogen, and fourteen equivalents of carbonic acid to obtain the carbon, while it retains also a portion of the oxygen. This can be illustrated in the following manner:—

Meconic Acid, $C_{14}HO_{11}$.



By this diagram it is seen that the Papaver somniferum, in preparing this organic acid, while it retains ten equivalents of oxygen which it absorbed with the carbonic acid, still yields to the air the balance, or eighteen equivalents of pure oxygen. Inorganic chemistry makes us acquainted with but three oxides of hydrogen,

namely, HO, HO_2, and HO_3. We observe the combination as existing in meconic acid as HO_{11}; such a union can probably only occur when the elements in question are under the influence of vital action.

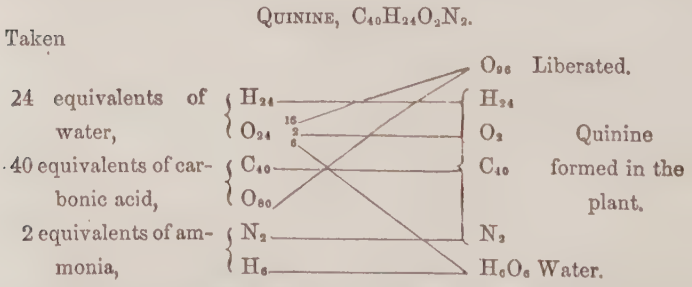
Attention thus far has been directed to the assimilation of binary and ternary bodies; it remains to exhibit the process of elaborating quaternary principles—principles which contain nitrogen in addition to carbon, hydrogen, and oxygen.

This class embraces two kinds of proximate principles, viz., first, the alkaloids; second, the protein compounds.

The *organic bases* contain generally but one equivalent of nitrogen. They are usually found in plants, associated with the vegetable acids as salts. While the acids, as we have just seen, abound in oxygen, the vegeto-alkalies are deficient in that element, none of them containing it in sufficient quantity to form water with their hydrogen, and some contain not a single atom. As examples may be cited morphine, $C_{34}H_{19}O_6N$, strychnine, $C_{42}H_{22}O_4N_2$, nicotine, $C_{10}H_7N$, and conine, $C_{16}H_{15}N$.

If these principles are directly formed, plants, in assimilating them, give to the air not only all the oxygen which they absorbed with the carbon as carbonic acid, but also a large amount of that which had been combined with the hydrogen as water in the plant.

In illustrating the assimilation of this class, quinine may be taken as a representative, its formula being $C_{40}H_{24}O_2N_2$. The plant, in forming this base, appropriates twenty-four equivalents of water to procure the requisite quantity of hydrogen, forty equivalents of carbonic acid to procure the necessary amount of carbon, and two equivalents of ammonia to obtain the nitrogen. This may be represented by the following formula:—



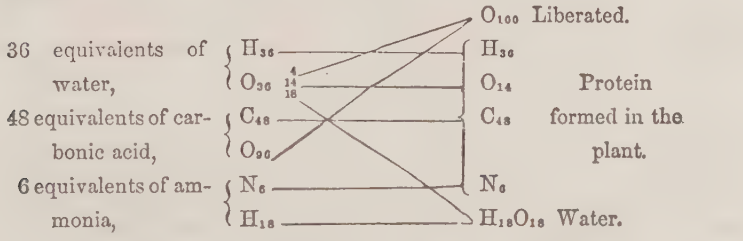
It is seen by this diagram that the cinchonas, in preparing this most valuable organic alkali, give to the air ninety-six equivalents of pure oxygen, and the same method of exemplification might be employed to show the formation of all the principles of this class.

It may be questioned whether the alkaloids are directly formed, for the reason that botanists, having been unable to assign them any use in vegetables, have regarded them as excretions. They cannot, however, be looked upon in the same light as the animal

excretions, for it is an established fact, that in plants there is "no decomposition and recomposition of any completed fabric." It is possible that they may be formed from the unappropriated elements which plants have liberated in the formation of other bodies. Dr. Gray, in speaking of the alkaloids, says they "are principally formed in the bark and leaves. We do not know that they bear any part in vegetation, nor of what use they are to the plant. In these substances reside the most energetic properties of the vegetable, considered as to its action on the animal economy—the most powerful medicines and the most virulent poisons. That they are of the nature of excretions," he adds, "may be inferred from the fact that a plant may be poisoned by its own products, introduced into its ascending sap."

The second class of azotized proximate principles includes fibrin, albumen, casein, and glutine—principles containing sulphur and phosphorus in addition to carbon, hydrogen, oxygen, and nitrogen. These bodies have been looked upon as containing a common base denominated protein, its formula being $C_{48}H_{36}O_{14}N_6$. In elaborating these principles, plants can procure the sulphur and phosphorus by deoxidizing the soluble sulphates and phosphates absorbed from the soil; in preparing the base protein, thirty-six equivalents of water are appropriated to procure the hydrogen, forty-eight equivalents of carbonic acid to procure the carbon, and six equivalents of ammonia to obtain the nitrogen. To illustrate:

PROTEIN, $C_{48}H_{36}O_{14}N_6$.



In assimilating these principles, therefore, plants liberate one hundred equivalents of oxygen, viz., ninety-six absorbed with the carbon as carbonic acid, and four from the decomposition of water.

We have thus briefly endeavored to exhibit, in a tabular manner, the direct formation of several of the binary, ternary, and quaternary principles, and those we have given may be regarded as typical of all the others.

Though it may be questioned whether all proximate bodies can be directly formed, it is somewhat extraordinary that the composition of these principles enables us to illustrate, in each instance, primary production by the same method.

While we are aware of the occurrence of various indirect formations in plants, it is difficult to decide which principles can,

and which cannot, be primarily produced. If direct formation is limited to the soluble dextrine only, in this case, as we have seen, the air is enriched by forty-eight atoms of oxygen for each atom of dextrine elaborated—in other words, by each atom of oxygen the plant has absorbed with carbon as carbonic acid.

Many principles which, as ordinarily observed, are insoluble, may in the vegetable juices be soluble, and consequently the range of direct formation may not be exceedingly limited.

Finally, though primary formation may be confined to a few principles, it is still possible to exhibit in a tabular form the transitions of principles directly formed into other bodies with the evolution of oxygen. To do this, however, requires a complicated *series* of formulæ, and it is obviously impracticable, on such an occasion, to present them in a satisfactory manner.

STATED MEETING, DECEMBER 17, 1868. DR. A. C. POST, PRESIDENT, IN THE CHAIR.

PROF. J. C. DRAPER described a modification of Breguet's thermometer, in which the compound bar is coiled in a flat spiral like a watch-spring, and is supported in a watch-glass which is open below, so that when it is placed on the skin there is nothing to interfere with the direct action of the heat on the spiral. Where it is required to introduce the thermometer into cavities, as the rectum, the coil should be enclosed in a thin silver globe or cylinder.

DR. WM. C. ROBERTS read a paper on "Causes of Disease and Mortality."

CAUSES OF DEATH AND DISEASE IN THIS METROPOLIS.—READ DEC. 17, 1868.

MR. PRESIDENT:—It is within the recollection of all of us that on Sept. 14, Dr. S. Rogers read a paper before the County Medical Society of this city, on Neglected Causes of Infant Mortality. There was in that paper much that seemed to me to be true and practically important, as well as interesting in a merely scientific point of view; and many of his views coincided with those which I had myself announced in papers read before this Academy. I had intended to take part in the discussion which was to follow, but the opportunity was not given to me. I have analyzed this paper, and propose to make it the basis of these remarks. The subject is the cause or origin of certain endemic, or endemico-epidemic diseases, chiefly occurring among the lower classes, and the means of preventing them; a subject of deepest interest to the sanitarian and philanthropist. In what I shall say I desire to be understood as in no way depreciating the labors of the Board of Health, whose earnest zeal I greatly admire, and whose efforts I would gladly see more extensive and successful.

Among these neglected causes Dr. R. places Heat, and by this

term "neglected," means, I infer, to say that its effects, as a cause of disease, are neglected, inasmuch as the means of overcoming them are neglected, which, among a certain class of the community, must necessarily be so, in respect of ablution, &c. But it is too well known among us all, rich and poor, high and low, to be generally neglected. It appeals too forcibly to our observation to be so. Heat is unquestionably, at all ages, among the young especially, a potent cause of disease and mortality, which are one and the same, for their connection is inseparable. The one leads to the other. It is, during the summer and autumn, the great cause of the congestive, diarrhœal, and dysenteric diseases, by which, among all classes, and especially among the young, mortality is so greatly induced and augmented. It acts, he says, by depressing the vital forces; by producing harassing and exhausting cutaneous disorders. As these, however troublesome and annoying, are harmless in themselves, I cannot consider them as inducing mortality, any more than the pest of flies, bugs, and mosquitoes, which the hot weather so plentifully engenders. It acts, indirectly, by deteriorating much of the food given to young children; souring and otherwise impairing the nutritive properties of the subsistence given them—milk more especially. This may be true to a certain extent among those to whom such deteriorated food is injudiciously given; but few are so ignorant as to use such food to any such extent as greatly to induce disease. It may also be said that summer is the season of fruit, to the injudicious and excessive use of which, ripe and unripe, much sickness is undoubtedly owing.

Lastly, and by far the most important, is the influence of heat in generating malarial agents, which adults, as much as infants, respire, and to which very many diseases owe their local origin, and some of which assume epidemic proportions. Intermittent fever, bilious remittent fever, yellow fever, *Asiatic cholera*, cholera infantum, cholera morbus, diarrhœa and dysentery, perhaps even typhoid fever, are in this large category. In all tropical, and even in temperate climes, heat is a powerful agent in the causation of disease.

In India it is presumed to be the exciting cause of Asiatic cholera; a disease which always exists there in summer, although it only, at longer or shorter intervals, extends thence epidemically, and involves the whole world in its ravages. The frequency with which hepatitis, abscess of the liver, diarrhœa and dysentery are induced by it and prevail in that country is a notorious subject of observation, and it is very much the same among ourselves.

Heat operates directly by causing, as I think, over-heating and expansion of the blood, and pressure upon the great nervous centres, enfeebling or destroying the force of the heart's action. Sunstroke is a striking example of this, and so are convulsions and meningitis in children, or pneumonic congestion, which I have seen fatal in a few hours. But oftenest its action is indirect, by caus-

ing and hastening the process of animal and vegetable decomposition, which is prevented or checked by cold, and thus producing putrid and septic malarious agencies, aided by certain meteorations conditions.

With regard to the production of intermittent fever, which prevails chiefly in spring and autumn, and requires for its engenderment the additional element of moisture, its influence is undeniable as well as indirect—that is, not operating *per se* alone upon the human organism.

To produce paludal malaria neither a perfectly dry nor a flooded land surface suffices. The land must be in process of drying; moist and marshy; or, if not marshy, at least moist and porous, in order that, in connection with the character of the season, the moisture, the temperature and the aërial movements of the atmosphere, intermittent fever may prevail. Level plains of sand, with perfectly dry surfaces, where no vegetation exists, provided that underneath the soil be percolated with water, suffice to generate ague most extensively and fatally, as the result of a specific poison, but one which possesses no extra-corporeal zymosis. So much for intermittent fever, which ceases in winter. Now as to yellow fever. Whatever may be the sources of malarious decomposition abounding among us, they do not produce yellow fever here. Always it has been an imported disease, and now, for more than forty years, it has not prevailed among us. But, in the countries where it is engendered *de novo*, having, as it has, certain geographical ranges, and now admittedly contagious, heat is its main producer, sources of malarial emanation being presupposed to exist. This endemic, in low districts on the seacoast, rarely occurs above an elevation of 2500 feet above the sea-level, never propagated beyond 48° N. Lat., nor without a temperature of 72° at least. Our sanitarians need have no fear, apparently, of its generation among us from internal causes, but when it comes it must be carefully watched, and stamped out as quickly as possible.

Cholera is another disease, in the production of which, and perhaps, though not always, certainly, since it prevails in mid-winter, in the propagation of which, heat has something to do. Cholera as an epidemic is never generated here. A few sporadic cases may now and then occur in summer, having the peculiar features of cholera; some of which may arise from lurking seeds of dormant contagion; others arise only from intensified causes, or are intensified cases of common cholera morbus, described many long years ago by Shakspeare and by Sydenham as existing in England, and probably elsewhere, in hot weather. But it does not propagate. To do this the seeds must be imported. No cesspool that ever stank, unless it contained the seminia of epidemic cholera, derived from the fœcal evacuations of the sick, ever produced, or will produce, epidemic cholera. But, when it is introduced, it requires for its diffusion certain atmospheric and terrestrial local conditions, and is capable of diffusion over the surface of the earth

both by aërial means and human intercourse, having, as Jos. M. Smith admitted, a meteorations and contagious principle, or element. To prevent this diffusion, *in every way*, becomes a most important object of sanitary science, and to a great extent it has been attained. Its entrance among us has been repeatedly interrupted, and its prevalence lessened, by the wise provisions and energetic action of our sanitary advisers.

Its germs are now considered to be indigenous in England, and probably elsewhere, but, fortunately, as yet it has not, like small-pox, exerted its independent power of zymotic extension, although this, at some later day, may be the case. It is now admittedly contagious, at least through the medium of the excreta, as I said that, being a specific zymotic epidemic, it must necessarily be. The same remarks are applicable to it as to small-pox, typhus, or typhoid. It may be communicated by the excreta, chiefly, however, the alvine, as typhus and typhoid are; by fomites, as they and small-pox are, only the superficial excretions are less directly contagious, although this even is not denied. The predisposing causes, filth, misery, and intemperance, are the same, but less peculiar to the class among which they prevail.

Dr. Aitken inclines to think that epidemic cholera is but the re-appearance of pestilential disease among us (English) in a form familiar to our ancestors as an annual visitor, and that now it has become indigenous. If that were true as regarded England, it would not hold good here, and Dr. Aitken forgets that the disease when it reached England, or occurred there as an epidemic, had advanced by progressive stages from India over Europe. No such epidemic had ever been noted. The local cholera of every year assumes no such proportions there or here. We then may surely adopt Dr. Harris' remark: that while the materials were smouldering in England, the flame which threw the mass into combustion has been of Asiatic origin. Why should it not be propagated by aërial progression and diffusion alone, or along with human intercourse? No cesspool ever produced epidemic cholera until it had first been impregnated with its germs. The facts, as we know them, authorize an adherence to the old adage, "venienti occurrite morbo," and teach us to neutralize the sources of its subsequent propagation.

Cholera morbus, we are told by Dr. Clymer's edition of Aitken, is a disease of hot climates, and the hot seasons of temperate climates. It is essentially different in its nature and origin from Asiatic cholera, and more specific. Emanations from cesspools, sewers, and putrilage (as I shall again have occasion to say), have produced a train of symptoms identical with those of cholera morbus.

I once saw an endemic cholera morbus break out in West Point, when I was there in the latter end of August, 1839, from causes seemingly purely local, and having, as far as I know, no connection with filth, &c., which ran in many cases into dysentery. It

may be that in all countries where cholera Asiatica has existed, dormant germs of specific poison may exist, and, becoming developed, may cause sporadic cases, either such originally, or causing the indigenous cases to assume that character, as dysentery sometimes also does.

I am surprised that in his enumeration of the causes of infantile mortality in New York, Dr. R. has laid no stress upon the frequency, annual occurrence, and mortality of what we call "cholera infantum," under which head, in the mortality reports of each year, from June to October, are included a very large number of deaths, from 3 or 6 months to 2 years and upward. Horner calls it a disease peculiarly American, but Trousseau says it has always been observed in all countries under similar circumstances of season, &c. Others again say it is unknown in Paris. Sydenham says there is a "sort of cholera morbus exceedingly fatal to infants," but certainly in England it is not known as the scourge of infancy and dread of mothers that it is here. Still I should think it would occur elsewhere.

It is an endemic of annual prevalence in the Middle and Western States, more than in Europe. I should say chiefly on seaboard—being the chief cause of the great loss of life among infants in the large towns of those States during the summer months, of special nature, characterized by typical symptoms and dependent on malarial causes; heat, &c., in some, perhaps, in malnutrition, occurring in and during the period of dentition; but seen equally (not perhaps numerically as often) among the rich as the poor; in children who have no teeth and in those who have many or all; commonest in July and August and decreasing in September, to its final cessation as an endemic, and by no means confined to great cities. The following statistics show this:—

1867—3d	week	in	June	23.			
" 2d	"	"	July	172.		1868—	84.
1867—3d	"	"	"	278.		"	138.
" 1st	"	"	Aug.	144.		"	163.
" 2d	"	"	"	135.			
" 4th	"	"	Sep.	26.			

The total number of deaths from cholera infantu min the 12 months ending September, 1867, was 785. Of these

535 under 1 year.	16 2 years.
224 1 year.	Under 5 years, 785.

Every one knows that as soon as hot weather sets in, the bowels of young children of all ranks begin to be disordered. The child becomes peevish, irritable, feverish and thirsty, refusing the breast or bottle, and craving cold water; if old enough to eat substantial food, losing its appetite. If confined to the breast or bottle, the milk curdles and is vomited in coagula with an acid smell. We say that the child has a sour stomach, or that its milk sours on its stomach. Either coagulation takes place too soon, or the gastric

juice is not secreted soon enough, and then the milk is forcibly and precipitately ejected soon after it is swallowed, or coagulated, sometimes with great sickness, nausea, and exhaustion. These are bad cases; emaciation is rapid and speedily fatal, often ending in eclampsia. The bowels become loose, and frequent passages occur of greenish or yellowish mucus, with appearance of whitish coagula, which may, for anything I know to the contrary, be curds of undigested milk. There is much flatus and tympanites, and the mucus is often striated with blood, and there is a tenesmus, which is the nearest approach I know of to dysentery in the very young child. Neither have I ever seen anything in the pathological condition of the intestines analogous to that of dysentery in the adult. The condition is rather allied to that of typhoid fever in the condition of the follicles, and they are, I believe, generically related. The reporter of the Board of Health says that the term cholera infantum is truly a misnomer, as it fails to indicate any pathognomonic fact that characterizes the malady; a remark as true of "typhoid fever;" it is, he says, a kind of death by starvation. It is truly a marasmus; but by what is it caused? It is, indeed, difficult to make the distinction between infantile diarrhœa, dysentery, and cholera morbus, unless the vomiting be considered a distinction, and they are no doubt often confounded, each reporter using the term which best accords with his own idea of the pathology of the case. The larger number of deaths in hot weather is recorded under the heading cholera infantum; but besides this, a large proportion of deaths annually occur from diarrhœa and dysentery, many of which are among children. Now, a few words more as to the causes of cholera infantum. The action of heat in causing cholera infantum, although positive, is indirect. But it is not alone in its action; for, says Dunglison, in country situations, where the heat may be equally great, in Virginia, for instance, it is comparatively rare. "It does not occur, to anything like the extent at least that it does here and elsewhere among us, among the poor of large European cities." A union of heat and vitiation of air of some sort, and an influence of locality, must be combined to render it universally and fatally prevalent. "Hot weather and filth," says the Report of the Board of Health for 1866, "combined, originate the thousands of *odors* that vitiate the atmosphere and destroy these delicate beings." Certainly not the odors alone, but the gaseous emanations which give rise to them; nor does this apply to country localities or the homes of the rich. That cholera infantum should prevail most extensively among the children of the poor, who live in ill-scavengered slums, among an intensely degraded and over-crowded population, in foul and ignoble wards, amid filthy streets and gutters, and insufferable nuisances which (Board of Health Report, 1868) are deliberately charged with the "slaughter of a large quota of deaths;" who are fed from the breasts of devitalized mothers, surrounded by septic influences, with a milk imperfectly elab-

orated and unfitted for food; or with a deleterious and inadequate diet, exclusively farinaceous or gelatinous, or with milk from diseased animals, and so diluted as to be unfit for food; who breathe a septic atmosphere, poisoned without by the putrilage of slaughter-houses, soap and glue factories, cesspools and sewers, and within, by emanations from the human body, filthy bedding and clothing, and often the excrement of man and beast (to say nothing of their overflowing numbers), should suffer most and most severely from cholera infantum, is not strange. All this is very fine and graphic. But it is not they alone who live among these vividly portrayed horrors who suffer from it, but the children of persons placed in circumstances exactly the reverse; children of the rich, well fed, cleanly, and comfortable, and in places where no such horrors exist. There is not, perhaps, during any summer, a young child in this city who does not suffer more or less from diarrhœal disorder; not a family with young children able to get away, that does not go into the country to escape the scourge, or who do not afford to them other available means of change of air and mitigation of temperature. In saying that cholera infantum is caused by age, high temperature, humidity within doors, according to Stewart, and mal-nutrition, the first two of which only apply in these cases, Dr. Clymer seems to me to overlook one great cause of development: malaria from terrestrial emanation, which operates elsewhere than in crowded tenement-houses, to give it an endemico-epidemic prevalence.*

It seems, according to Dr. R., that the greater cleanliness of the 4th, 6th, and 10th wards did not save them from an increase of 100 per cent. of deaths. Many a quiet, retired, shady and cleanly country town or village will exhibit, I believe, during the hot weather, a high sick and death rate from this cause, to say nothing of the dysenteries, typhoid fever, and intermittent or remittent, with which we often know them to be infected. I quite agree with Dr. R., that carbolic acid and chloride of lime are not all that is required, however beneficial in themselves, to prevent this annual mortality; nor is even change of air always sufficient. This, with the avoidance of much that is wrong in diet and regimen, and the doing of much that would be right and proper, is just what the poor cannot attain to; but, even under the most favorable circumstances, children at this and even later periods of the year will sicken and die, and so maintain the rate of mortality popularly assigned to them, whether statistically accurate or not I do not know—one-half in the first year of existence. In their over-crowded and ill-ventilated close quarters, those of the poor must remain and make the best of it;

* Cholera Infantum.—The reader will find, in the *N. Y. Med. Gaz.*, 1841, a series of papers entitled "Contributions to the Literary History and Pathology of Chol. Inf.," by the author, containing a complete and condensed account of all that was known, up to the period, on the subject, and some original autopsies and thoughts on the nature and characteristics of the disease. W. C. R.

and certainly, where there is the most fuel for an endemic there it will rage with greatest intensity and fatality. In reference to this subject, Dr. Elliot truthfully says: "All the world over, among the most favored classes, where is the family that has succeeded in raising all its children, even under the best conditions and with the best medical advice? Do what we may, we cannot lay the spectre of the inevitable mortality of childhood." "Every summer for the past 20 years," says our Reporter (ay, and for twenties of twenties before that, say I), "has witnessed a fearful slaughter of infants in New York," and will again, I fear, until better means and accommodations are provided for the children of the poor.

If, then, even the healthier, cleaner, better clothed and fed and circumstanced children of the wealthy yield to the influence of these combined telluric and atmospheric agencies, let us inquire what are the causes of sickness among the children of the poor, apart from heat and malaria, the odors and foul emanations from drains, sewers, &c., which at no other seasons of the year are capable of producing similarly disastrous results.

First may be ranked the nervous susceptibility of the young child, owing to its rapid growth, the predominance of blood in the brain, predisposing it to convulsions and meningitis, and, if you choose, the influence of dentition, its liability to cold, to which in winter it is as much exposed as to heat in summer, from imperfect clothing and warming, leading to broncho-pneumonia and croup; the limited space it occupies, and the vitiated air it breathes in close and crowded tenement-houses, impairing the due oxygenation of its blood, and checking its growth; add to these defective nutrition, and the peculiar liability of the young to the zymotic infections, the possibility of prematurity of birth and syphilitic infection, and the strumous diathesis, and the reasons are sufficiently obvious why, when the rich cannot keep their children well, the poor should not. All these are causes of mortality among the poorer classes, and the upper often as well, sufficient to account for it without superadding the effects of filth, &c., which certainly does not alone engender very many of the diseases to which the younger population of our great cities succumb.

The great underlying cause of much of this evil among the poor is poverty, which brings in its train all sanitary and social evil; of which more anon. But I may be permitted here to remark, that if indeed, while the 20th and 22d wards were never more cleanly and free from internal nuisances, the streets and gutters, as elsewhere, reek with filth and insufferable nuisances *to which a large quota of deaths is owing* (which I do not believe), little will be gained by internal sanation until this evil is abated under competent jurisdiction. If the filth of the streets, gutters, and garbage boxes be indeed sources of origin of specific diseases, and the cleansing of such be not, and I believe it is not, under the supervision and control of the Board of Health, with full power to enforce

it, the sooner it is put there, or some harmony of action be obtained, the better: for until then, if such be the state of the streets, &c., so often alluded to, one-half at least of the evil will remain unremedied.

I come now to a question, the consideration of which was a main inducement to my writing this paper; a question of scientific accuracy in relation to the etiology of diseases and their mode of development, whether spontaneous, or otherwise: interesting apart from all theory or preconceived ideas of sanitary reform, although intimately connected with it. Like every other, it has two sides, and on each are ranged able advocates. Here let me say that filth, the stenches which it originates, the causes on which they depend, popularly and in law termed nuisances, as they certainly are, so far at least as the senses are concerned, is the *bête noir*, the bugbear of modern sanitarians. Filth originates stenches: nuisances create stenches: decomposition of some sort is at the bottom of all, and malaria is the consequence. "The 1st ward," says our reporter, "is crowded with filthy and improvident people. The chief *constant cause of epidemic* diseases in the ward is filth in the domiciles, with utter want of ventilation; in the streets, gutters and sewers." "Typhus is said to be an offspring of personal and domestic filthiness," &c. Here we see that filth is assigned as the cause of the various diseases, specific or otherwise, which swell the annual mortality: and with Dr. Rogers, with all due deference, we cannot but think that this reasoning is incorrect and unscientific, and that these so-called (aërial) causes of disease are by far too much emphasized by health officers: the grounds of which belief will gradually unfold themselves. I further agree with him in saying that it may be well enough to seize upon an appreciable stench and insist upon its removal from beneath our nostrils; but it must first be shown that such stenches originate disease. This is the question to be decided.

Do stenches and nuisances really, of themselves, or rather the effluvia, results of the decomposition which create them, really, of themselves, engender sporadic diseases, or diseases of any other character? and if so, what are the diseases which they do engender? are they, in themselves, specific causes of specific diseases, enumerable as such? are they, in themselves, properly pestilential, containing specific poison, capable of producing pestilential disease? How far are they injurious to health and detrimental to life, or only comparatively innoxious nuisances and to be abated *as such*. The former is the view taken of them by most sanitarians in this and other countries; notably by Mr. Simon of England, who stands pre-eminent for zeal and science, and whose authority it is hard to oppose. Filth is constantly referred to by our Registrar as a cause of disease. He tells us that most of the fevers, diarrhœas, and dysenteries are caused by bad air, impure water (consequence of filth), insufficient food, and ill-ventilated dwellings, and he calls them preventable maladies. *Consumption* even is ranked among these, although known

to be often connected with an hereditary predisposition, a lymphatic temperament, seen alike in hovels and in palaces, among rich and poor; and far from being oftenest even caused, or even fostered, by the above-named causes of insalubrity. Others, again, doubt if typhus has any other origin than in contagion, and believe it to be of as specific a character as small-pox, &c., which Dr. Harris admits are not so caused.

There are some, men of undoubted authority, who do not believe that a filthy drain can alone engender typhoid fever, which they consider equally specific, unless it shall contain the specific germs of the disease, derived from the excreta of a typhoid patient, in a state of decomposition and zymotic, which heat has a great influence in developing; who think that other affections, notably cholera morbus, dysentery, and diarrhœa, caused by stenches (or their causes), are very rare, limited to a very few susceptible persons, and possessed of no specific character whatever, seldom extending beyond the individual, while in most cases, even when concentrated, they are wholly innoxious.

The opponents of the theory that offensive smells, or the emanations which produce them, and arise from decomposing garbage (animal and vegetable, effects of chemical decomposition of various sorts). are productive of specific diseases, are, 1. Dr. Watson; 2. Dr. Graves; and 3. Dr. Budd, of Bristol, who has persistently, and with great ability, opposed all suggestions of the possibility even of the origin, *de novo*, of *any* specific disease; and lastly, Dr. Aitken. Each of these gentlemen shall speak for himself in this abstract from their respective writings on the subject.

Dr. Watson does not believe that continued fever (including typhus and typhoid) can be generated by effluvia constantly proceeding from the healthy human body under circumstances of over-crowding in filthy, close, and unventilated places. He instances, as proofs of this, the absence of the disease, properly so called, in the huts of the natives in the arctic regions; slave and convict ships; the Black Hole of Calcutta, &c., in none of which is it known to occur. Jail typhus, he says, never originated there from filth, defective ventilation, and human effluvia, but was always imported; and Howard also found the same to be the case in the prisons of the Continent, just as foul and filthy. It is rapidly propagated there, but never engendered. Quoting Bancroft, he says that neither the putrid air of dissecting-rooms, nor the noisome effluvia from foul and ill-conducted burying-grounds, nor those to which tallow-chandlers, soap-boilers, glue and catgut manufactories, and the melting-places of whale blubber, nor the foul air of privies and sewers, have ever been known to produce continued fevers. Large shoals of sprats, spread over land as manure, produce a horrible stench, but no disease results. Dr. Chisholm (Ed. Med. Jour., vol. 6) brings forward other and very satisfactory evidences to the same purport; a bone manufactory;

one for converting the flesh of dead animals into adipocere; leather-dressing; manufactories for refining sugar; all tending to the conclusion that air, deteriorated by the decomposition of animal matters, is not necessarily noxious to life, and still less productive of continued fever. Diemerbroeck says that in 1642 a great slaughter occurred, and that at least 8,000 soldiers were slain, besides many servants, rustics, wagoners, women and children, besides an innumerable quantity of horses; their bodies remained unburied and putrefying for a long time—*tamen nulla pestis orta est.* To some of the above-named sources of putrid effluvia let me add manufactories of printing-ink, about the most offensive of any; poudrette manufactories—the abattoir of Montfaucon, near Paris, and the slaughter-house tainted streets, Mott, Mulberry, and Elizabeth. The remarks of Dr. Graves, of Dublin (see Med. Jour. for February and May, 1849), in reference to this subject are directed particularly to the causation of epidemic cholera; but they are equally applicable to that of other epidemic contagions of specific character. He ridicules the idea that sewers, streams, rivers, and damp localities are to be blamed for producing cholera, which, engendering no disease during the hot unhealthy months of summer and autumn, suddenly, in the months of October, November, and December, are claimed to become endowed with a power of manufacturing noxious vapors capable of infecting the neighborhood with a new and specific disease, a consequence, he says, which only leads to a despotic government of the whole country by sanitary committees, whose medical police, not content with the triumph gained by flushing sewers, whitewashing houses, abating nuisances, emptying dust-holes, and performing hyper-augean labors, will persecute water wherever they find it. He derides the idea of keeping cholera out of Dublin by enforcing cleanliness, and thinks that unless *perfect* success be attained, it were better, upon an old and familiar principle, that nuisances lie dormant than to stir them up into fresh activity. Means for the obtaining of increased comforts are, he justly says, of primary importance.

Chilled and crowded together, the poor will still present meet fuel for the epidemic, nor will their hunger be appeased nor their cleanliness increased by their being informed of the cleansing of a neighboring alley, nor the flushing of an adjacent sewer. He maintains that heat and dampness have too long exerted their influence on various localities, and have been powerless to generate new and specific diseases. He tells us that in the last cholera of Italy, the Ghetto at Rome, inhabited almost wholly by Jews, which is remarkable for a concentration of every nuisance, whether solid, fluid, or gaseous, that is capable of producing disgust or causing disease, wholly escaped.

If, then, says Dr. G., notwithstanding unflushed sewers, stagnant cesspools, noisome dunghills, and all the countless abominations which render the quarter a by-word even among the Romans,

themselves among the filthiest of Christians, such a locality was spared, can cholera, or any other specific disease, be produced *anywhere* by those agencies on which the advocates of dirt and want of drainage as the cause so confidently rely?

He ridicules the idea that the same sewer which is *falsely* supposed to generate typhus in June and July, in August to vomit forth hooping-cough, in September and October, small-pox, and in January, scarlet-fever, should now add the new and specific disease, cholera. He says that the condensation of wretchedness never can originate *specific* diseases, whatever its malign influences may be. And he adds:—

"I cannot give my assent to the benefits that are supposed to accrue from opening the sewers, and whitewashing the houses in the poorer parts of the city. It is true that obstructed sewers give rise to disgusting nuisances. *But*, the causes of epidemic disease escape the scrutiny of both nostrils and vision, as is proved by the fact that the worst parts of most capitals of Europe, however abounding in all sorts of abominations, do not give rise to either *typhus*, or plague, or cholera.

"Filth is the outward and visible sign of *poverty*, which is more injurious to health than dirt, entails sporadic diseases, and, by the crowded state in which the poor must necessarily live, *increases* the spread of contagious disease.

"That this may be prevented, humanity, in its visits to haunts of misery, *must pluck the inmates from* within these bounds, disseminate them over a larger space, where the same number that now inhabit *rooms* may occupy *large houses*, and may have the use of nutritious food, fuel and clothing.

"But, alas! this—the ONLY *sure method of relief*—will require something more expensive than the broom and brush."

"Filth *alone*," says Prof. A. Clark, "does not cause cholera." And in another lecture he says: "Overflowing or uncleansed privies, cesspools, soil-pipes, imperfect ventilation, personal uncleanliness, open sewers, and similar causes of ill health or sickness, do not aid in multiplying the cases of typhus, scarlatina, small-pox, and measles." Certainly they do not, unless by increasing predisposition, and certainly they do not engender them; and unless they do engender typhoid fever in some rare instances, it remains to be seen hereafter what diseases they do engender.

In the British Medical Journal, August, 1868, I find the following observations, relevant to the subject under consideration:—"Extreme heat of weather, incautious use of unsuitable drinks or food, &c., may *bring on diarrhœa*, but, it would seem, in the case of cholera, that some special poison is present which produces the characteristic symptoms of the disease." And again: "As a further illustration of this subject, we can take the *popular fallacy*, that smells are injurious as such. There can be no doubt that it is unwise to breathe a vitiated atmosphere. A disagreeable smell may cause nausea, or irritation of the alimentary canal, or it may de-

prive the surrounding atmosphere of a certain amount of oxygen, and replace it with gases deleterious to respiration. The smell from a tallow-chandlery is by no means pleasant, or that proceeding from gas-works, but *it is not* unhealthy. On the other hand, persons may be attacked with ague by living in a fenny country in which no smells abound; and in the case of zymotic diseases, the poison enters the individual and no smell is noticed. It is, in fact, important to make a distinction between bodies which are decomposing and capable of generating disease, and those which only produce disagreeable smells by generating harmless gases. In the former case, as in cholera, it is the germs which are generated that are the actual causes of evil, and not the smell that accompanies them. Thus it is that bad smells may produce no disease, while it may be caused by material undergoing change without giving rise to any foul-smelling product. A smell, then, so far from being injurious, rather only warns us of possible danger, as it may arise from decomposing matter containing specific germs."

"There is no fear of cholera, unless seeds of contagion have once appeared. Excessive heat, bad smells, or deficient water-supply will not cause it, though when it breaks out they propagate and generate fresh doses of poison" (in other words, favor zymosis). Too much attention, then, cannot be given to sanitary measures of prevention, by means of such disinfecting agents (carbolic acid, &c.) as arrest the growth of organic matter. In reference to this subject I may cite some observations from Dr. R.'s paper. "It does not appear," he says, "by the records, that the gas-house localities and soap-boiling regions are particularly insalubrious." "We have taken pains to inquire extensively into the health of people employed in various disagreeable odored occupations, and find no facts to support the popular and fostered belief that a stench is necessarily deteriorating to health or destructive to life." "There is, lastly, no reason why bad drainage alone should suddenly nearly double the death-rate, when the temperature for weeks before such sudden mortality was high enough for the production of more or less rapid decomposition of garbage and miasmatic exhalation."

To this array of testimony on the part of learned, cautious, and able men, Dr. Aitken adds the weight of his authority. He tells us first, on the authority of Alison, that during the cholera in Edinburgh in 1848–9, although most of the subjects dissected in the University had died of cholera, no case occurred among the students engaged; which is certainly very remarkable, since these bodies must have reeked with specific germs in a state of decomposition. Next he says, that animal effluvia produce conditions of ill health which induce *a proclivity* to the specific communicable diseases. That in large towns the sewers are constantly charged with the *materies morbi* of specific diseases. In small villages and other places where no sewers exist the *air* only may be infected, or the water contaminated by the direct or indirect importation of cases of specific disease, or by the person itself; so that the organic im-

purities, the dung-heaps, the open soil which surrounds the dwellings of the patients, the cesspools and privies common to several houses, gradually, but eventually, become impregnated with the specific poison of the disease, and hence its rapid spread. He says that "in endemics of typhoid-fever, the first case in the series may have been casual and imported, or due to a rekindlement of some dormant germ of a former one. That the decomposing alvine discharges, dried or liquid, being thrown into a water-closet or privy, the drains, or system of drains, become at once saturated with the specific poison"—excellent reason why the drain should be disinfected, but not showing that a foul drain, *per se*, can originate the disease. Dr. Budd believes that fermentation, putrescency and decomposition, even as predisposing causes, have been much overrated. And Aitken, on reference to Murchison's pythogenetic theory of typhoid fever, thinks that there does not seem to be sufficient evidence to show that filth, foul drains, &c., alone, can produce a disease of so specific a nature as to be maintained and propagated by a specific poison generated in the body alone. While, on the one hand, we have high domestic authority for saying that the chiefest constant *cause* of epidemic diseases in a filthy ward is filth in houses, streets, gutters, and sewers, Dr. Simon, more cautiously, only affirms that from such sources of atmospheric vitiation, and chiefly that which is excremental, cholera, &c., derive their epidemic destructiveness.

Such are the views taken on one side of the question; on the other, Anstey, a candid and intelligent writer on epidemics, believes that there is much evidence to show that over-crowding does generate a miasm in destitute persons, capable of exciting typhus: the theory of our late friend, Prof. J. M. Smith: and Murchison is of a similar opinion. But, let me insist, famine is a far more potent cause of typhus than filth—personal, in its various forms, or domestic, whose action it only intensifies.

Now as to what diseases filth does engender, other than specific. 1st. Aitken—Animal effluvia produce ephemeral febrile states and proclivity to specific disease. 2d. Cullen—Effluvia from very putrid animal substances readily produce *diarrhœa*—very rare in a clean dissecting-room. Anstey says that it is well established that epidemic diarrhœa, a constant product in England of autumnal seasons which succeed to a long continuance of hot, dry summer weather is caused, *in part*, by the effluvia from decomposing animal matters, and he cites one very remarkable instance in support of his views. Large quantities of sewage matter, supplied by the house, was profusely scattered as manure over the fields near the windows of an asylum, the gases from the decomposition of which entered the house and produced an outbreak of severe and fatal dysenteric diarrhœa.

This may be so; but we know that such things have happened before and no such results have followed. The sanitary condition of Milbank Penitentiary was very bad; but until the diet was

changed and reduced, no diarrhœa resulted. In another instance Dr. Anstey says, that in the Newington Workhouse, of London, a very large and open tidal sewer, that stank offensively in hot and dry weather, was for many years a source of diarrhœa and dysentery among the inmates. Similar diseases are believed to originate in tainted drinking-water from leakage or drainage, or in some other way. This might even be true in London, where they drink water out of decaying wooden butts; but how does it apply here, to houses supplied by the Croton?

Diarrhœa and dysentery seem very common sources of mortality in this city. Thus, for the first nine months of 1866, the total deaths of both sexes, from

Typhus	was	371		
Typhoid	"	362	Dysentery	549
Scarlatina	"	564	and from	
Diphtheria	"	334	Diarrhœa	1,004
Measles	"	144		
Total,		1,775		1,553

Not all of these cases could be referred to the influence of heat or animal decomposition, because many of them occurred at seasons when this agent was inoperative.

Some of these cases assume the typhoid aspect in a general sense, some in a specific sense. Now whence was this infection derived? Did it occur *ab origine*, or was the poison inhaled during the course of the disease aërially, communicated by personal contagion, or in some other way introduced into the system? You are all aware that many sanitarians believe in the *de novo* origin of typhoid and typhus fever; while others hold a contrary opinion, and believe that they never occur unless by specific contagion. The question is still an open one. Yet if these two diseases, themselves specific and developing specific germs, can originate *de novo*, why may not scarlatina and small-pox, cholera, and so forth? which is not claimed for any of them. Again: can a foul drain originate at one time a specific typhoid fever, and at another a non-specific dysentery, or any other of the specific diseases at other times, and under certain diverse circumstances?

But, if the drain, sewer, or cesspool become contaminated with the decomposing specific egesta of a disease, say typhoid fever, typhus, or cholera, we can readily understand that from it germs may issue while decomposing, and entering the body either of the sick or well, by the air from imperfect traps, from the soil, in tainted water drank, &c., produce (engender) in it the specific disease which they represent. It is difficult to conceive of any other way in which these diseases are brought about. Stinks, then, and effluvia, are harmless to produce specific disease, unless containing the germs of that disease.

Petenkofer believes that the penetration of the soil by the *discharge of cholera patients* was the first essential link in the

chain of propagation; and the discovery which conferred so much honor on the memory of the late Dr. Snow, of London, was that the sewage from a neighboring house-drain had leaked into the well, in Broad Street, *into which the discharges of a patient residing in the house in question* had entered, and gave rise to the sudden explosion of cholera in that neighborhood, in Aug., 1854. Mere foulness of drains, then, you observe, does not alone give rise to cholera. Budd, the antagonist, with Watson and Graves, of any origin of specific disease *save from contagion*, admits that the excretions of TYPHOID FEVER patients, by contaminating the soil and drinking-water, may and do cause the reproduction of the "germs" which he supposes necessary to the "propagation," I should say, production, of the disease. Thus, then, we see that the pathogenetic theory of Dr. Murchison does not stand on a wholly immovable basis. Even our own Board of Health does not commit itself too positively on the subject of typhoid. (Report for 1866, p. 139.) "Its localization," it is said, "and the *probability* of its production by foul sewers and filth-sodden ground, and the infective quality of the alvine excreta in this enteric fever, make it a duty to cleanse and disinfect the privies, drains, and premises where it occurs. This is unquestionably true. There can be no surer (perhaps no other) means of preventing the spread of infectious diseases, as typhus, cholera, and typhoid fever, than to destroy the infective specific germs which give rise to them, and the efforts to this effect now making here and elsewhere are commendable in the highest degree, and have been attended with the greatest advantage to the general health. But, not containing these specific germs, neither air nor water is infectious, and a foul drain or privy becomes a nuisance merely; only, when its odoriferous or organic elements are highly concentrated, and under certain circumstances, capable at most of inducing in a few susceptible persons a nausea or vomiting, or diarrhœal dysentery. Not that by this consideration the propriety of its evacuation and purification is in the slightest degree called in question. In furtherance of this idea we may cite a further passage from the Report before us: "The foul sewers, privies, and filth-soaked earth of the crowded city is ever a ready soil for *the deadly germs of this* enteric fever;" and Dr. Farr says, the impurities of the *seeds of the disease* can only be got rid of by a vigilant sanitary police. Other sources of fetid emanations than foul drains are, says Dr. Anstey, occasionally found; and he adds that he has known a whole family made seriously ill, and more than one of its members to suffer unmistakable typhoid fever, owing to the decomposition of the dead body of a single rat behind a wainscot or beneath a floor; a statement which I cannot receive without a certain degree of hesitation.

Let me briefly recapitulate what seems to be the truth in relation to these matters.

1. The odors arising from certain manufacturing establishments,

gas and bone-boiling, &c., slaughter-houses, &c., however disagreeable to the sense of smell, are in no wise pestilential, as inducing disease in the neighborhood, or in the persons employed in them. In a few rare instances, susceptible persons are affected with nausea and vomiting, and occasionally with diarrhœa or dysentery. As nuisances, they should be suppressed.

2. No filthy sewer, drain, privy, garbage-box, gutter, street, &c., is pestilential in any other sense than the above, unless it shall hereafter be shown that such, in a state of great concentration, under certain circumstances of heat and moisture, or even in winter, are capable of producing *de novo* typhoid fever—a point which is still undecided, and to say, the least, very doubtful. In many well-marked instances in which disease was believed to be induced from such causes, that disease was not typhoid fever, but a cholera-morbus, or diarrhœal dysentery. Typhus, although constantly included by English sanitarians as having similar origin, is not, so far as I have seen, distinctly attributed to any other cause than *encombrement.*

3. No drain, privy, &c., can induce or engender specific diseases unless they contain the specific seeds or germs of said disease, and emanating thence, or by soakage or infiltration of earth, or in some other way, these seeds shall find their way into the water drunk, or issue from soils impregnated with them, in which case, as in cholera, for example, specific disease may ensue. In view of this possibility, and their general offensiveness and tendency to vitiate the surrounding atmosphere, they should be promptly cleansed, &c. Inodorous gases are the most dangerous and truly pestilential; those which are mephitic are, for the most part, innocuous, serving rather as warnings of possible danger than as actual causes of disease.

It is truly gratifying to see, by the published Reports, how keenly alive is our excellent Board of Health to the dangers arising from this source, and how ably they are met and opposed, to the extent, I suppose, of its pecuniary ability. It strikes me, however, that in proportion to the extent and population of the city, and the vast numbers of tenement-houses in it, the sanitary inspectors are few in number, and capable, even with their best exertions, of giving only an incomplete and partial inspection of the premises. Many houses must remain unvisited; domiciliary visits must be few and far between; filth soon re-accumulates, and the work has to be begun over again. Two inspectors in every ward, at least, are needed, and even then we may say, "What are they among so many?"

I come now to what, beyond the mere attempt to establish scientific truth as to the causation of diseases by filth, stench, &c., is the main object which I had in view in writing these lines: and I shall take as my text, the remark made by Dr. Rogers:—

"We can see but one remedy for this desolating evil, and that is to improve the dwelling-places of these people." It is to the

credit of Dr. Rogers, that in his able, for the most part justly reasoned and humanely-intentioned paper, he has brought this subject before the notice of his professional brethren, assembled in this temple of science to deliberate upon the best means of effecting that great object of the medical art which brings men nearest to the gods—the giving of health, happiness, and longevity to our fellow-men. Carbolic acid and chloride of lime are not, indeed, *all* that are required to prevent the mortality, infantile and adult, among the lower classes in our city.

I have already cited Dr. Graves on this subject. He tells us that the only manner in which this can be effected, is to remove the poverty of the inhabitants of our filthy slums and overcrowded tenements. Filth is the outward and visible sign *of poverty, which is more injurious to health than dirt;* encourages indolence, despondency, and intemperance; leads to squalor, and entails in its suite insufficiency of warmth, ventilation, and nutriment, and, *worst of all*, over-crowding. Now, to remedy this evil, "pluck," he says, the inmates from these bounds, disseminate them over a larger space, where the same number which now inhabits rooms may occupy large houses, and may have the use of nutritious food, fuel, light, and clothing—a costly one indeed, but the only method of relief. Anstey says: "The evil influence of over-crowding is (in London) assuming gigantic proportions, and cannot, without much peril, be treated any longer on the do-nothing system. Either by governmental interference or by private enterprise, a large proportion of the operators who work in London and other great cities ought to be provided with proper and wholesome apartments, built in blocks, and furnished with sanitary appliances near the *scene* of their labors, or else induced to take suburban residences attainable by railway;" a most admirable suggestion for those who possess the means of its attainment.

From the Annual Report of the Board of Health for 1866, p. 155, I take the following: "*In no other city in the civilized* world are there to be found 500,000 people *so unhealthily housed, as a class, as the tenant-house population of New York.* To prepare for these *poor ignorant* persons (ever increasing in numbers), whose *poverty and ignorance allow no election of better homes*, such dwellings as the munificence of Mr. Peabody and the public enterprise of Alderman Waterlow have prepared for the London laborers' home, and to maintain ample and well-conducted lodging-houses, under strict police surveillance, in our metropolis, would be to multiply the domestic happiness and virtue of many thousand families, *and amazingly diminish the amount of sickness and death among the laboring* poor," without, *let me add*, needing much assistance from disinfecting agents.

Yes, gentlemen, this is, with no disrespect, but, on the contrary, warm and complete approbation of what the Board of Health is doing, under the wholesome prompting of our able and enthu-

siastic Registrar, whose scientific knowledge, courteous affability, indefatigable industry, and fervid zeal (I had almost said fanaticism) in the cause of sanitary reform all must gratefully and willingly acknowledge, the true remedy for the insalubrious condition of the poor, and its consequent sickness and mortality.

Remove or abate their poverty; supply them with better means of living, in healthy and commodious homes, wherein

> "The goodly light and air
> Are banned and barred, forbidden fare"

no longer: then you will have cheerful, sober men coming gayly home after their day's labor to loving wives and fond children; cleanliness and comfort will take the place of filth and misery; despondency and sickness will disappear without the aid of an enforced hygeia, carbolic acid, and chloride of lime.

This, gentlemen, is the true remedy for all this domestic misery. Let us hope that every one of us will give his best consideration, aid, and countenance to the means of bringing about this most desirable consummation. Then will

> "Peace and plenty cheer the laboring swain;
> The pallid cheek its healthful hue regain;
> Hygeia, pleased to find her labors o'er,
> Depart with shouldered broom to disinfect no more;
> No more vile stenches taint the ambient air,
> But all be clean and sweet and pure and fair."

And let me say one word in conclusion. When carbolic acid, and chloride of lime, and the host of other disinfectants have been lavished in the process, with more or less salutary effects, there are, after all, no substitutes for the cheaper ones which nature supplies, if they could only at all times be availed of—pure air and fresh water. Nothing will cleanse a foul drain or gutter like a good flushing with the Croton, which is too sparingly employed, and unfortunately there are too many houses in which this invaluable aid to cleanliness and health is deficient, and which by reason of their construction cannot be properly ventilated. When these defects are done away with, and these two supreme ministers of cleanliness and salubrity duly appreciated and availed of, the death-rate among the poor will, I hope, exhibit a corresponding diminution of mortality.

Dr. Worster exhibited an inverted uterus removed from a lady, and remarked as follows: I was first called to see the patient on August 28, 1868, by Dr. James Kennedy—found the patient very much exsanguinated—found attached to the uterus a polypoid tumor, about the shape and color of the egg-plant, which, after removal, half filled a chamber.

The operation consisted, first, in applying the écraseur around the centre of the mass, and cutting it in two parts; then came tumbling out from the centre of the mass hydatids, which were about the size, color, and consistency of white currants. I then

applied the écraseur to the balance of the mass, in close contact with the womb, and removed the rest of the tumor. Immediately afterward I left for the country, and saw no more of the patient for two months.

On returning I was informed that the tumor had been reproduced. I resolved on removing it immediately, which I did on the 29th of October, pursuing precisely the same process as we did in the first instance. Then applying the fangs of a Vulsellum forceps to the body of the tumor, and drawing it down in full view external to the vulva, I found to my astonishment we had an inverted womb, the history of which dated back 24 years. On further examination of the external walls of the organ as it then existed (everted), there appeared to be too much disease of the whole mass to be allowed to remain with any degree of safety to the patient's life. We resolved to remove it entirely, and applied the écraseur to the upper part of the uterus, that is, above the cervix, so as to involve the whole body and cervix of the womb, and, drawing it down firmly, with the écraseur removed it entirely in a very few minutes. Then came down two or three folds of intestine. I replaced them by passing the hand, seizing the folds of intestine, passing them up gently into the cavity of the abdomen, where they seemed to remain without any tendency to return. Having no needle forceps at hand, we concluded to give her the chance of adhesion, by supporting the edges of the wound from falling into the vagina too far, by slightly packing with cotton, and thus left her, directing her to preserve the recumbent position. In five days afterward there was complete adhesion. There were no symptoms of peritoneal inflammation whatever; the vagina had united in a complete cul-de-sac. In a month afterward she was able to sit up without further untoward event. She had been lying on her back for the previous eight months.

Appearance of the womb afterwards: As soon as it was removed, I, with a pair of scissors, split the organ antero-posteriorly from the cervix to the fundus, revealing the broad ligaments, Fallopian tubes, and ovaries singularly concealed in the cornu of the inverted organ. There were several hydatids embedded in and about the mucous membrane of the uterus.

STATED MEETING NEW YORK ACADEMY OF MEDICINE, JANUARY 21, 1869.
THE PRESIDENT, DR. A. C. POST, IN THE CHAIR.

After a few remarks the retiring President introduced his successor, Dr. H. D. Bulkley, who proceeded to address the Academy.

Dr. Wm. Detmold then read the following paper on

PLASTIC OPERATIONS, ILLUSTRATED BY CASES.

I had intended merely to offer a few remarks upon the subject of a plastic operation where I intended to introduce a somewhat

novel plan of operating; but as I am able to produce here to-night several cases in point, I shall take the liberty of somewhat extending my remarks.

Plastic surgery took its origin in rhinoplasty, as orthopedic surgery did in the operation for club-foot, and for a long time plastic operations were limited to the nose; in fact, it took its origin upon the general principle in political economy that the demand will produce the supply. We find the first traces of rhinoplasty in the East Indies, where it was not only a common punishment of certain crimes, such as adultery, to cut off the nose, but where it was also customary, in the wars between the different tribes, to cut off the noses of the prisoners of war: thus the demand was established, and the supply followed. The priests there have been for centuries in the habit of restoring noses from the skin of the forehead. Through English travellers this became known in England; but the operation does not seem to have found any followers in Europe until the same principle of demand and supply asserted itself there. When, after the discovery of this continent, the gold of Mexico poured into Spain, the people became demoralized, and syphilis spread almost like a pestilence over the land, especially among the armies in the Franco-Hispania wars in Italy, so much so that thence arose the mistake that syphilis was of American origin. The disease was not only widespread, but it was also very destructive, partly perhaps because its treatment was not understood. Thus a great many noses were lost, and this induced Tagliacozzi, a Professor of Anatomy in the University of Bologna, to make the attempt of repairing the lost noses, and he was so successful that a marble statue was erected to him, which represented him holding a nose in his hand. His method is still known as the Italian method, in contradistinction to the India method. After that there have been isolated cases of rhinoplasty in different countries, until Dieffenbach of Berlin devoted his attention to these operations, and now plastic surgery forms, like orthopedic surgery, almost an independent branch of surgery, a specialty. Thus much for the history of plastic surgery. Without entering into minutiæ, we may divide our means of supplying material wherewith to restore lost parts into two classes, that is (1), by transplantation of flaps of skin, either from the neighborhood or from more distant parts; or (2), by merely shifting or sliding the skin, which the French call glissement. I shall presently show you cases where these different modes come into play.

Before proceeding further, we must remember one fact of the utmost importance in all plastic operations, and that is, that although almost all other tissues of the body, even nerve tissue, will be reproduced, skin never is reproduced. Where there is loss of skin, the loss is made up either from the yielding of the neighboring skin or by new fibrous tissue, which is, however, not skin; it has neither the structure of the cutis, nor has it any hair or sebaceous follicles, and, like all new and adventitious tissue, it is liable

to become atrophied and to contract. Wherever we have, therefore, loss of skin in the neighborhood of the eyelid, or the mouth, or the nose, we find them drawn out of shape and place by the contraction. Thus we have in this case ectropion. Where the loss of skin occurs on the body or limbs, we have the parts firmly contracted, as you see in this case on the neck. Both these cases are the results of burns, a fruitful cause of deformities calling for plastic operations. In the third case, here you see the lower part of the nose destroyed by lupus: the septum and both alæ are gone, and again, in consequence of the contraction, you see the nostrils closed.

Now without giving you all the different methods by which these deformities might be remedied, yet I think in giving the modes aforesaid, which I mean to adopt in these three cases, I shall be able to give to those not familiar with this branch of surgery a fair view of its resources.

1. Let us take first the case of entropion: it might be remedied by transplanting a flap of skin from the neighborhood. I shall, however, adopt a simpler plan. I shall make a V-incision, dissect the flap off, and make a Y-scar, thus pushing the eyelid up.

In the second case—the loss of the lower part of the nose—I shall take the septum from the upper lip without twisting it, but turning the mucous membrane out, which will soon be changed into cutis, and take the alæ from the cheeks. Where there is only a small part of the nose lost, I prefer taking the material from the cheeks instead of from the forehead. For the restoration of the entire nose Dr. G. Buck has of late years performed some heroic operations with brilliant success, by taking the skin from the forehead. As far as I am aware, the method of taking the skin from the arm has not been followed in this country. A few years ago I had a case where the entire nose was lost by lupus, and the forehead being by scars unfitted for transplantation, I had prepared everything for the operation of making the nose from the arm; but a day or two before the operation the man found a woman who was willing to marry him without a nose, and he therefore thought the operation unnecessary.

The third case is an extensive burn of the neck and chest, where you see here the contraction. I have seen this peculiar form of deformity repeatedly, and to an even greater extent, and in most cases it was caused by the child upsetting upon itself the boiling water from the pan which is generally kept on the stove. In this case it is caused by the child's clothes taking fire.

This case might also be remedied by transplanting a flap from the back, but it would cause a very extensive wound, and the success is doubtful. I propose to perform first the same operation which I have just described in the case of entropion. I shall try to recover the chin by making a large triangular flap, in fact by turning a V-shaped incision into a Y, and to relieve the contraction I mean to draw from the width of the skin and to turn it length-

ways, by what I consider a new method. I shall make either one large or several smaller transverse incisions; then, by stretching the neck as far as possible, and making the wounds gape wide, I shall by sutures change the transverse incision or incisions into vertical ones, and thus gain in length. On this case I shall operate on Wednesday next, at three o'clock, in the amphitheatre up stairs, and shall be happy to see any of the fellows who take an interest in the case.

At a later day I shall bring these three cases again before the Academy, and show the result of the operations.

NEW YORK ACADEMY OF MEDICINE. ADJOURNED MEETING, FEBRUARY 11, 1869. DR. HENRY D. BULKLEY, PRESIDENT, IN THE CHAIR.

The special order for the evening was a discussion upon

THE INFLUENCE OF EMANATIONS UPON THE HEALTH OF CITIES.

Dr. JOHN O. STONE opened the discussion by appointment, and spoke substantially as follows:—

This subject, Mr. President, was brought to our notice a few meetings ago, in a paper read by Dr. Roberts. In that paper I found very little to object to. It had evidently been dictated by the proper spirit, and its whole object seemed to be to inquire into the truth of the subject, and to do it justice. But, while finding little to object to in what the paper said, I have to regret that it did not say more; and in spite of its excellent tone, I am sorry that it was written. For I think the Doctor has failed to do justice to the hygienic knowledge of the day, and I fear that his sober argument may tend to strengthen the fault-finders, who are perpetually troubling the Board of Health, throwing difficulties in the way of its removal of nuisances, and backing up the rich men who delay its action. As soon as any one thinks his rights of property invaded by any action of the Board towards the abatement of a nuisance, he at once comes forward, and brings forward physicians to testify that there is no nuisance. There are the shell-lime burners, for instance. By the burning of the oyster shells the atmosphere of the whole neighborhood is filled with carbonic acid, and yet physicians are found to declare that this is not a nuisance. So with the butchers. Their establishments were scattered over the crowded parts of the city. The floors were leaky, and the cellars and sewers were filled with putrefying blood and excrement. Still, physicians would come forward and say that these things did not injure the public health. Even after the judicial decision in our favor, the only way we could get rid of these slaughter-houses was to agree not to prosecute their owners if they would remove their business beyond Fortieth Street. This they have all done. I say that I fear Dr.

Roberts' paper is likely to give countenance to these people. I confess, also, to some personal sensitiveness upon the subject, for in my connection with the Board of Health I have entertained the hope that I might do some good; and to find my cherished convictions assailed, or even made light of, by implication or innuendo, leads me to review my position, and seek whether or not I may have been wrong in assuming it. I had thought that, in entering the Board, I might do some good to the profession; that I might, at least, show them what were the great impediments to recovery from disease, and what was one of the great causes of disease. I had thought, moreover, that I might do some good to the public. I felt that if I could aid in improving the condition of things in the wretched tenement houses, I should not only be saving life and health, but also be doing much to prevent the poor laborer from frequenting the grocery and becoming the victim of intemperance, for often his only excuse for this is that his dwelling is so comfortless, and its atmosphere so depressing, that he has to resort to the liquor shop for relief; and if we could thus save these heads of families, their numerous children would be better provided for; they would cease to be so familiar with crime; would be shielded from the temptations to its commission, and so would grow up into respectable citizens, instead of becoming a burden to the State in its almshouses and jails. If you can raise a healthy population, you have done the best thing possible for your country, for it is upon such a population that the country must depend for strength in war and for labor in peace. But if the laboring class is overcrowded in tenement houses, under unhealthy conditions, it will inevitably become so weakly, and withal so degraded and mean, that a large part will either die prematurely or grow up to fill your almshouses. The great question of pauperism is intimately involved in that of the sanitary condition of this class.

Dr. Roberts' main idea seems to be one based upon the cell-theory—that the emanations noxious to health are those which have a cell-growth, like that in yellow fever, in pneumonia, in the cattle-disease of last summer, and, he would add, in scarlatina, measles, etc. Now I am very fond of the cell-theory; I believe there is more truth to be developed from that idea than from any other view before the professional mind. But, while admitting the cell-theory as fully as he does, I think the doctor should have taken one step beyond this; he should have recognized that the gases which are generated in the crowded parts of a large city are injurious in themselves. The cell-theory is thought to be true because we have seen so much in its favor; but there are other things, of which we have positive knowledge, which the doctor, in his paper, has ignored; and this seems to me a very grave omission. He takes no account of the exhalations from the manure and offal of the streets, of the emanations from factories, etc., or of the great mass of persons that we know are affected in

health by these emanations. It is certainly not proved, as yet, that the cell-theory can be made to include these causes of disease; it is not proved, though it may be possible, that the gases arising from these various sources may develop, in the body, the very cells which he claims as characterizing disease. Let us not lose sight of what is well determined in our pursuit of what is still theoretical. I repeat, that if the doctor had only taken one step further, and dwelt with more prominence upon the fact that disease may be produced by emanations that have not yet been shown to have anything to do with cell-developments, we should not differ.

That there is good reason why the various gaseous emanations to which I have referred should be injurious, I will endeavor to show; and this is my main object in this discussion. Atmospheric air consists, as you know, of very nearly one part of oxygen to four of nitrogen, with a variable admixture of carbonic acid gas (two to four volumes in a thousand), derived from different sources. An excess of this carbonic acid is the great thing we have to meet in our efforts at sanitary improvement. We all know that a mouse, placed under a bell-glass, will poison itself to death by exhausting the oxygen and producing carbonic acid. We all know the story of the "black hole" of Calcutta—the fearful mortality that ensued, in a few hours, from the confinement of the prisoners in a small room with no ventilation. The cell-theory will not account for these results. We have no reason to suppose that any cells entered the blood of these prisoners, to produce such terrible destruction. But some poison did so enter. That poison was carbonic acid; and, from its deadly effects in such aggravated cases, we may learn how harmful it must be wherever it is found in excess. Many persons have believed that consumption and scrofula are due more to this gas than to any other cause. But, be that as it may, I wish to say that the deterioration of the air by gases is, in itself, a cause of disease not sufficiently acknowledged.

Look at the tenement-houses. See a large family crowded into one or two rooms—in winter, the windows nailed down and every cranny stopped up; the same air breathed over and over again by eight or ten persons; the red-hot stove consuming oxygen still faster, and sending out, through its very substance, the fearfully poisonous carbonic oxide, and perhaps also sulphuretted hydrogen; add the steam and smoke from the cooking; the exhalations from the unwashed bodies of so many persons; the gases from the sink or water-closet, trapped imperfectly or not at all; and very likely those from the children's evacuations kept in the room—and you have a picture, not overdrawn, of the dwellings of thousands in our city. If it be summer, the windows are thrown open only to admit the fever-breeding emanations from overflowing privies and cesspools, and heaps of decaying garbage in the yard. What wonder that these people die in

great numbers; and that, even where there is no specific affection engendered by such terrible surroundings, they are so enfeebled as to fall an easy prey to disease when it appears? When scarlet fever or typhoid fever comes among them they have no strength to withstand it, and no fair chance for recovery. Even if convalescence does drag its slow length along, it is so very slow that some other disease will, most likely, overtake and stop it short. I regard pure air, then, as of more vital importance than food. In this country a man in health can get a living well enough; but if his vital powers are exhausted by breathing impure air he cannot eat, he cannot work. Bad air and poverty go together.

Our school-houses, too, which we are all so proud of, look at them. I have not a word to say against our excellent system of public education; but I do think our school-houses a discredit to our civilization. In these rooms—over-crowded, over-heated, the atmosphere deoxidized and tainted with the exhalations of hundreds of bodies—the children sit with hot skins, hot heads, and perhaps cold feet, from the bad system of warming, and their delicate nervous systems tortured by the fear of punishment; and here they are kept from nine o'clock till two. It is enough to crush the life out of them; and they do die, or become so weakened that they can never regain robust health. From these hot rooms they go out into the cold air, their relaxed skins not braced to meet it, and pneumonia or some other disease speedily carries them off. These facts we have represented to the Board of Education. The new school-houses are, many of them, good; but the old ones have been so surrounded with buildings that there is no way for fresh air to reach them.

Next we come to the hospitals. You no sooner enter one than you are greeted by an odor indefinable, made up of exhalations from close stools, from the bodies of the sick, from suppurating wounds, from tuberculous lungs—and here the cell-theory may come in, for the contagiousness of consumption is still an open question. Add to these, cases of typhoid or typhus, or of any of the exanthematous fevers, and you have an atmosphere which is intolerable, and which makes it almost impossible for patients to recover within any reasonable time. Go a step further, you have erysipelas; another step, hospital gangrene. What causes these? We know one thing, it is a poison, soaked into the walls, into the floor, into every part of the wards. Cleanliness will not get rid of it; tearing down the walls may possibly do so; but the best thing is to put your patients into tents. There is no doubt about it; there is something in the atmosphere of these wards which, in its concentrated form, is deadly, and produces the effects I have just named. Less concentrated, it produces the minor effects of headache, hot skin, exhaustion of the nervous system. The amount of effete matter to be found in these hospitals is very great. In the Hôpital St. Louis the

sweepings of one ward were found to contain thirty-six per cent. of animal matter, and those of another, sixty-six per cent. Thrown into the fire, it burned like hair; moistened, it gave off a most disgusting odor. This is not simple epithelial waste, though that doubtless makes up a considerable part of it; and it may contain some of the very disease-cells that we have had described to us.

We come now to speak of the decomposition of dead bodies. Although we do not bury in the city, still we have suffered much from this cause—in the dissecting-rooms; in the butchers' shops before alluded to, pouring their blood into the sewers; in the bone-boiling and fat-frying establishments. These nuisances we have been trying, for three years, to drive from the populous portions of the city; and we have just succeeded. Even after they were professedly closed, it took twenty policemen, with Mr. Schultz at their head, to ferret out and effectually suppress the night-working of some of these places. When scattered wide by the winds their emanations may do little harm; but when concentrated they become most noxious. The whole of Abattoir Place is filled with manufactories of this kind,—one man trying out guts for the fat, another working in the same material to make fiddle-strings,—a long row of houses loading the air with the most abominable stenches. The only thing we can do is to grant no new permits; and when a place is once closed, to keep it so.

To show how virulent may be the gases produced by decomposition, take one or two historical examples. M. Chambon, with four pupils, was directed to make a dissection of the liver in a subject so far gone in putrefaction that the pupils hesitated to go on with the demonstration; but the Dean persisted in requiring it. One of the pupils fell down on the spot, and within seventy hours was dead. Another was seized with a violent cutaneous eruption. Two others remained ill for a long time afterwards, one of whom never recovered his health.

Could any cell-growth have acted in this violent manner? It is impossible to imagine it. There must have been some gaseous emanation of more than ordinary toxic power—bisulphide of carbon, or something like it—which killed this man within seventy hours.

In 1713, at Dijon, by the accidental breaking of a coffin which had been buried six weeks, 114 out of 120 persons who were present at this grave were violently attacked with illness, and eighteen died.

The exhalations from a badly closed tomb at the church of Saulière caused illness to sixty-six children who were present, of whom twenty-four died, as well as the curate and vicar.

It takes something more than the cell-theory to account for this.

At the church of Santa Maria, in Lucina, the effluvium from the dead bodies in its vaults is such, according to Mr. Nathalis

Guillot, who visited it, that any one remaining in the building a short time is likely to contract severe fever. Whether any of the tenants of the vaults had died of fever, I do not know, but they had been buried for many years.

Let us next consider the emanations from the sewers. M. Gaultier de Claubry says that the air of sewers has its oxygen diminished by from one to four per cent., and that its place is supplied by carbonic acid and sulphuretted hydrogen. Science has not yet come to examine this air with her microscope; very possibly she may find cells in it; but however this may be, the instinct and experience of the people often run ahead of her slow-moving steps, and prove better guides to follow. Now M. Duchatelet has made the extraordinary assertion that the air of sewers is not injurious. Doubtless he entered the sewers in favorable weather, and when they had been well flushed; and he tells the truth about what he found.

But the common people know well enough that they suffer from sewer-air. Under ordinary conditions it gives them headache and general malaise; and when it is overcharged with sulphuretted hydrogen and other foul gases, they find among its effects ophthalmia, giddiness, great nervous depression, gastric derangements, and perhaps fever. Whether sulphuretted hydrogen will of itself cause fever, I cannot say; some persons have thought so. Nor can I tell the exact amount of it necessary to destroy human life: one-three-hundredth part of it in the air is enough to kill a dog. It is when the contents of the sewers have been stirred up by heavy rains that you get an evolution of these gases which is terrible. The pressure is so great that they force their way through the traps and escape into the houses. Then, too, when the water from the roofs is conducted into the main drains, the water running below exhausts the water in the traps above, so that the gases may pour up without hindrance. Turn on your cocks and fill your traps anew is the remedy for this.

In this connection I would like to say something about the defectiveness of our sewer arrangements. Below the Park, even Broadway is not sewered. On either side of the city, where there is much made land, the sewers often end where the made land begins. This is a nuisance which the Board of Health has no power to correct; the city authorities will not do it; and we must wait until time has made the evil so intolerable that the public shall insist on its removal. Again, where the rivers are lined with wharves, many of the sewers empty into the slips, instead of being carried to the end of the pier, where their contents would be taken up by the swift current and diffused. When the tide goes down, the sulphuretted hydrogen is so abundant as to discolor the paint of the ships, and to sicken even the sailors and 'longshoremen. Still another trouble is, that the tide flows up many of the sewers to such an extent as to prevent any proper drainage. To remedy this, it has been suggested by the engi-

neer that an immense sewer be constructed all around the city, to receive the flow from the sewer-mains, which would be pumped out at the Battery.

One grand work, at any rate, we have done, and I think we deserve some thanks for it. There is no more driving of cattle below Fortieth Street; the streets are no longer defiled by them; and lives are no longer imperilled. All the slaughtering is now done above Fortieth Street; and we have some of the best slaughtering establishments in the world. One of the most complete of these is the fat and hide establishment at Forty-fourth Street and East River. Its facilities are sufficient for killing half the meat of the city. The charge for slaughtering and dressing is only the hoofs and horns. The blood flows, in tight pipes, right into the swift currents of Hell Gate; and everything about the place is a model of cleanliness. Other slaughter-houses show so great an improvement upon the old style that we have reason to congratulate ourselves that so much has been accomplished. But there is a hog-killing concern on Fortieth Street and North River, which, in one of its aspects, is really awful to think of. Four or five hundred hogs are killed there every night, and some fifty thousand in the course of the year. This is more than would suffice for city consumption, and much of the pork is packed for shipping. The work begins here about six or seven o'clock in the evening, and goes on all night. I think it takes about two minutes to kill and dress a hog. One man does all the sticking, and he has to be an artist in his line. The blood of these fifty thousand hogs pours into the North River. The current here is very slow, and the water shallow near the shore. The blood, therefore, is likely to be thrown against the shore, and become a stagnant mass of putrefaction. The establishment itself is admirably conducted; everything is kept with the utmost neatness; the steam boilers are all tight. The proprietors have shown a disposition to conform to our wishes, and have been treated accordingly. No one lives in the neighborhood. The only trouble with the place is the danger of serious results from the accumulation of the blood on the shores and flats below. So much does the Board of Health fear these, that it will give no more permits for killing in that direction. It means that this work shall all be done where the blood can be carried at once into deep water.

Two things more I would allude to: first, the immense omnibus stables in various parts of the city. These infect the atmosphere about them, and greatly aggravate disease when it arises. Some have thought that even diphtheria might be traced to their influence. The necessities of the city demand these conveyances, and for the present we must have horses to drag them. But the people are seeking substitutes for horse-flesh, and I believe that not long hence—within ten years or less—we shall have all our vehicles drawn by caloric engines, or something of the kind.

Lastly, I come to the greatest nuisance of all—the gas-houses.

We all know that, three or four years ago, these used to fill the whole city with their disgusting odors. The companies, though made up of men individually respectable, were utterly reckless. These establishments should not be allowed within the city limits, and so far as I can use my voice in the matter, it will be for their unconditional removal. They use coal abounding in sulphur, which, on removal, gives off quantities of sulphuretted hydrogen. One of the worst parts of the process is the lime-purifying, which keeps the lime-burners here—these two classes of nuisance-makers playing into each other's hands. The lime is not removed until it is supersaturated, and then it emits all the horrible odors you can imagine—sulphurous acid, sulphuretted hydrogen, ammonia, bisulphide of carbon, carbonic oxide, all of them most poisonous gases. If the wind is east, we get these spicy perfumes wafted from the works of one company; if west, from those of the other. A dispute has been going on between the two as to the best process of purifying, the New York Company using what is called the iron process. If there is any conclusion to be drawn from the pamphlets issued, it is that the nuisance is greater even than I had ever dreamed. The companies admit that the purifiers are carelessly cleaned, but say that they cannot control their workmen.

I have now opened the discussion, Mr. President, and will repeat what I said in the beginning, that I should have been most happy to agree fully with Dr. Roberts, whom we all know to be a man desirous of taking the broadest and fairest views of all questions; and if he had only carried his observations a little further, I should have been able to agree with him. It seems to me that there are morbid influences at work which generate diseases, the influence of which he has not admitted. The causes of typhoid fevers, for example, have not been demonstrated to be owing to cells or spores, and whether the bad influence of impure air on the blood is or is not a sufficient cause for their production is still an open question. But we need not dwell on this point, since every one admits that bad air aggravates diseases where they have arisen, delays convalescence, and from its depressing influence on the vital powers, permanently impairs the constitution, and must, in many instances, be considered to be the immediate cause of death. The Board of Health holds this position, that air, fresh air, is the all-important thing. Remove, so far as possible, everything that vitiates the air, and where it must necessarily be more or less vitiated, let in fresh air enough to supply the oxygen consumed, and to so far dilute all noxious exhalations that they shall become comparatively innocent. This view is the only one that can justify our course. Our practice is justified by our principles, and our principles are proved correct by the results of our practice.

Dr. Anderson had hoped that Dr. Stone would throw some light upon the National Hotel endemic.

After remarks by Drs. Bibbins, Hubbard, Foster, and others,

The Academy then adjourned.

STATED MEETING, FEBRUARY 18, 1869. DR. H. D. BULKLEY, PRESIDENT, IN THE CHAIR.

Dr. FRANK H. HAMILTON read the following paper:—

The operations which I have hitherto made for restoration of the nose are as follows:—

Operation by the Italian method—that is, by transplantation from remote parts.

Case 1.—Barnard, æt. 28. When a child he lost a large portion of his nose in consequence of a burn. The whole left side of his face was also extensively burned, leaving cicatrices which covered the whole surface of his skin, everting both the upper and lower eyelids, and closing the left side of his mouth.

In March, 1847, before the class of medical students at the Buffalo Medical College, I made my first operation for the restoration of the upper and lower lids. These were both successful. A few days later I enlarged both commissures of the mouth with equal success.

On the 11th of March, 1848, I attempted to restore the right ala by the French operation, sliding, and I was in a great measure successful.

March 29th, I commenced the operation of restoring the left ala and the end of the nose, by dissecting a flap from the radial margin of the ball of the thumb, which two weeks later I attached to the end of the nose. The attachment to the hand was severed at the end of 72 hours. About one-fourth of the flap sloughed, but sufficient remained to make an excellent nose.

In October of the same year, not feeling satisfied with the length of the left ala, near its top, I dissected a piece of skin from the thickened tissue within the nostril, and attached it to the front of the ala, adding thus two or three lines to its length. (For a full report of this case, with drawings, see *Buffalo Med. Jour.*, vol. iv., p. 459, *et seq.*) Dieffenbach, in his *Operative Surgery*, published in 1856, says that Labat has made a septum (*columna nasi*, I presume is meant) from the palm of the hand; but he does not approve of it, believing that it endangers inflammation of the aponeurosis. He raised his flap from between the thumb and index finger (vol. i., p. 350).

I do not know that any one else has ever attempted to restore any portion of the nose from the hand.

Operation by sliding—French method.

Case 2.—I have already stated that in the case of Barnard I constructed the right ala in this manner. The right side of the face being unscarred, I first made raw the right side of the nose, and the remnant of the cartilage surmounting the septum; then I made two horizontal incisions from the side of the nose, and extruding outwards about two inches, carefully avoiding the duct

of Steno. These parallel incisions were about one inch apart. The quadrilateral flap thus made was dissected up and made fast to the front and ridge of the nose.

Two weeks later I cut the base of this flap, to allow the cheek to fall back, and to relieve the strain upon the end of the nose. This incision was made a little convex outward, and at a point corresponding to the portion of the base of the ala. The result was complete relief to the strained flap, and a depression around the ala closely resembling that which is natural. The success of this operation was satisfactory both to the patient and myself.

Case 3.—E. B., of Chemung Co., aet. 11, in consequence of an extensive lacerated wound of the face, when 4 years old, lost a portion of the right ala of his nose, leaving a large hole communicating with the nostril, and at a point a little farther back the same nostril became completely closed by adhesion of its walls.

Nov. 30th, 1844, before the class of medical students, I excised the edges of the opening, and made two parallel incisions outwards to the extent of one inch and a half. This flap I dissected up, and made fast with six fine silk sutures. These sutures were reenforced by adhesive plasters placed obliquely over the face, nose, and forehead.

On the twelfth day I dissected the base of the flap in the same manner as in the case of Barnard.

The success was complete, except that the nostril, which I had opened with a trocar, and had attempted to keep open, became again closed, and the ala remained a little drawn down.

Two years later I opened the nostril again, and introduced a bag stuffed with lint. I also removed a crescentic piece of integument above and to the outer margin of the ala, and brought it together with sutures.

I have never seen the patient since, and do not know the result of these last operations.

By torsion, and from the forehead,—the Kooma or Indian method.

Case 4.—March 30th, 1852, I operated at Brockport, N. Y., upon P. Davis, æt. 64, for an epithelioma on the side of his nose, situated between the ridge of the nose and the left eye. Present—Drs. Clark, Carpenter, Thatcher, Woods, and others. The disease appeared thirteen years before, and in the mean time a cancer doctor had once removed it by caustics. The ulcer was now nearly two inches in diameter and exposed the bone.

I removed with the knife the edges and base of the sore freely, and about one-half of the lower lid; also I cut away all the bone which appeared diseased. From the centre of the forehead I dissected up an overflap, leaving a broad pedicle, and made it fast in the original site of the ulcer with fine silk sutures, laying over the flap a thin dressing, with cotton batting above, and secured the whole with adhesive plasters.

I did not see Mr. Davis again, but I was subsequently informed that the flap united quickly and thoroughly; but that after the lapse of some months the disease reappeared in the bone, and eventually he died of the malady.

This operation was delayed too long to be successful; when the bone is invaded by epithelioma, it is, in my experience, quite as likely to return as true cancer.

Case 5.—Feb. 3d, 1857, I operated upon Dr. S. Ward, a gentleman of large practice, residing at Silver Creek, N. Y., removing an epithelioma which involved the whole of the left ala of the nose. The integument was taken from the forehead and attached to the margin of the wound, leaving a long pedicle. Union occurred speedily, and in about two weeks I cut the pedicle, replacing it in the wound not yet closed in the forehead. The result was restoration of the ala so completely that after a few years when I saw him again, the fact that an operation had been made could scarcely be recognized. The color of the integument was natural.

Case 6.—Aug. 26th, 1852, I operated on Miss Parmele Parmenter, æt. 33, of Busti, Chautauqua Co., N. Y., for an epithelioma on the side of the nose. Its history was as follows:—At birth there was a small red spot, or nævus, near the inner angle of the right eye, below the lower lid. When 27 years of age it began to "twinge," then it became more elevated and more red, crusts formed and fell off, and finally it became an open ulcer. When I operated it had invaded the side of the nose.

I operated as in the preceding case, taking integuments from the forehead. The wound made by excision of the ulcer was about one inch and a quarter in diameter. I cut the pedicle and laid it back on ninth day, and September 20th she returned home, the cicatrization being complete. I have heard from her since, and have seen her photograph. The cure is complete, and no deformity remains.

Case 7.—James Gleason, æt. 50, has had a wart on the right side of his nose, near the inner angle of the eye, twenty years. In 1864 it began to grow, and bleed occasionally. Finally it fell off and left an ulcer. When admitted to ward 11, Bellevue Hospital, in Feb., 1868, the ulcer was about one inch in diameter.

Feb. 11th, 1868, I operated, cutting away the margins and base of the ulcer, and supplying its place with integument from the forehead, and making this fast in my usual way. A very small portion of one margin of this flap sloughed, owing, I think, to its having been subjected to undue pressure after being laid down.

Feb. 22d, eleven days after the first operation, I cut the pedicle and returned it to the forehead, almost completely filling up the wound I had made on the forehead.

Within a few days this man has called on me, and the result is a complete success.

Case 8.—Sept. 15th, 1855, Dennis Dasy, of Medina, N. Y., æt. 30, had a large piece of the end of his nose bitten off in a fight. By his request a surgeon put on the piece, and sewed it fast, five hours after. It did not unite.

Sept. 22d, seven days after the mutilation, I operated, in presence of Drs. John Boardman and Spearman, of Buffalo. Freshening first the wound on the nose, I made a flap from the centre of the forehead, and turning it upon itself, secured it to the end of the nose with fine silk sutures.

Oct. 5th (13th day), I cut the pedicle and replaced it in the forehead.

Oct. 10th and 15th, some little repairing was done to the alæ, and soon after he was discharged with a very respectable nose. I have never seen him since.

Case 9.—This is the case of the woman which I reported to the New York Pathological Society, February 13, 1867, and which is published in full in the New York *Medical Record* for May 15, 1867, at p. 137. She was an Irishwoman, and her nose had been bitten off by her negro paramour on the 1st of January, 1867. About one inch and a quarter of the end of the nose was taken away, and also a portion of the septum and alæ. I operated at Bellevue on the seventh day, by a flap removed from the forehead, and in my usual way, leaving a long pedicle.

One month later I was preparing to divide the pedicle and lay it back, when she was killed by chloroform administered for the purpose of making the operation.

(Dr. Hamilton presented the head of this patient, with the nose precisely as it was left by his operation.)

Case 10.—Maria Calligan, æt. 24. When 4 years old the ulceration which finally destroyed her nose commenced. At the time of the operation the septum was entirely gone, and several of the small bones of the nares had been removed. The end of the nose, as far back as the ossa nasi, had fallen into the nares.

Oct. 24th, 1868, I made the first operation, by excising freely the edges of the vestige of nose, describing as well as possible by my incision the original outline of the base of the nose. Having previously constructed a pattern from pasteboard, this was laid upon the centre of the forehead, with its base upwards, and a line was drawn outside of the pattern with a brush moistened with tinct. of iodine. As soon as this was dry I cut my flap, including the columna, by following the outer margin of the stain, thus making allowance for subsequent contraction.

On the right side the incision terminated at the base of the ossa nasi, on the left it descended one inch lower. The flap, being raised, was twisted upon itself and made fast with fine silk sutures to the abraded surfaces below. The columna was not fastened, lest in contracting it should draw the end of the nose down.

Dec. 16th, 1868, about eight weeks after the first operation, I

made the second, cutting the pedicle between the eyebrows, refreshing its lower surface, abrading the skin of the nose over the nasal bones, and stitching it fast at the top and along its margins.

(This patient was brought before the Society and exhibited.)

Rhinoplasty by other methods.

Case 11.—A young man fell from a two-story window, striking upon his face, and breaking the cartilage forming the ridge and the septum nasi. The consequence was that the ridge of the nose remained permanently depressed just where the cartilage attaches itself to the end of the nasal bones, and the end of the nose was tilted upwards and backwards, causing a serious deformity.

One year after the accident he consulted me. I introduced a narrow, sharp-pointed bistoury through the skin of the nose on the right side, and resting its edge upon the ridge at the junction of the cartilage with the ossa nasi, I cut the cartilaginous septum directly backwards about three lines, and then, making a gradual curve with my knife, I cut downwards about eight lines towards the end of the nose. The intercepted portion of cartilage could now be easily lifted with a probe, and the line of the ridge of the nose completely restored. It was at once apparent, also, that lifting the cartilage would depress the tip of the nose, and restore its symmetry.

To retain the cartilage in place, I constructed a gutta-percha splint of the length and shape of the nose, but so formed along its middle that it would not press upon the cartilage which I had lifted, resting well upon the ossa nasi, but not touching the ridge from the lower ends of these bones to the tip of the nose, at which latter point it again received support. I now passed a needle, armed with a stout ligature, through the upper end of the uplifted cartilage, transfixing, of course, the skin on both sides of the nose, and this I tied firmly over the splint. This accomplished the important object of pressing backwards and downwards the tip of the nose, and thus tilting up the upper part of the ridge and septum, and of more effectually securing the cartilage in place by lifting it directly with the ligature. On the second day the ligature was removed, but the splint was continued two weeks, during most of which time a band was kept drawn across the lower end of the splint, and tied behind the neck.

To prevent the cartilage from falling back when final cicatrization occurred, I pressed the sides of the splint firmly towards each other, just below the incision, so as to force as much as possible the walls of the nares into the fissure of the septum made by lifting it up.

The result is a complete and perfect restoration of the nose to its original form.

Case 12.—Margaret Jones, æt. 34, having lost by ulceration the entire nose, leaving a large opening communicating with the

nares in front, she was placed under my charge at Bellevue Hospital for an operation.

I proposed in this case to make the nose also from the forehead, but before doing so I determined to close the opening in front, which was made by the destruction of the nasal bones, and which was large enough to admit my thumb. My object was to supply a base upon which the new nose should rest, and thus to secure for it greater elevation.

Accordingly, on the 13th of Jan. last, I made elliptical incisions on either side of the chasm, the two incisions commencing at a common point about one inch above the chasm, and from thence diverging downwards and outwards to points on either side opposite the lower extremity of the nose. The integuments interposed between the incisions and the margins of the opening were then dissected up and folded over toward the centre of the opening, and here united by silk and wire sutures, leaving the tegumentary surface downwards and the raw surface upwards. The success of this operation is complete, the opening being perfectly closed, and a true tegumentary structure being nearly completed.

(A cast representing the original condition of this patient, and the patient herself, was presented, showing her present condition.)

Case 13.—Richard Tracy, of St. Catherine's, Canada West, æt. 21. June 13, 1855, following an attack of fever, he had a severe nasal hemorrhage, to arrest which his surgeon stuffed his nostrils with cotton-batting, saturated with the muriated tincture of iron, crowding both nostrils very full. It smarted severely, and the nose became much swollen. Two days later it was removed. The result was a sloughing of the columna nasi and of a portion of the septum, with one of the alæ.

August 3d, 1855, in presence of Professor Austin Flint, Jr., I released the attachments within the nostrils, and raised the end of the nose with a tent. I am unable to describe this operation more fully, having no further notes of it, and not being able to recall the particulars. It was not, however, very successful.

September 18th, in presence of Professor Flint, Professor Mason, of Buffalo, and others, I dissected a long, narrow quadrilateral flap from the ridge of the nose, and twisting it upon itself, I brought it down and made it fast to the upper lip, so as to form a columna. This operation succeeded admirably.

October 10th, I repaired the ala from the side of the face by sliding integument. The final result was a very great improvement, but not a complete restoration of the original form. The loss of a portion of the septum rendered this impossible.

Case 14.—July 2d, 1866, Dr. Luther Voss, of this city, operated on Eliza Cromie, by transplantation from the forehead, and covering up in a very skilful manner a hideous chasm caused by the loss of her nose. The result was a full success.

Subsequently, there being no vacant bed for her in the hospital at

which Dr. Voss is on duty, at her request she was received under my charge at the Charity Hospital.

I have made two operations upon her eyelids—blepharoplastie—and endeavored to make one or two changes in the form of the nose. Without difficulty I reduced its breadth and length a little by removing a V-shaped piece from its extremity. I conceived also the idea of elevating the centre, by causing the inner and lateral walls to adhere. For this purpose I scarified the inner surface of the new nose as well as I could by means of a narrow sharp-pointed knife, thrust through the side of the nares, and then placed on either side of the whole length of the nose a plate or splint of gutta-percha, and secured them in place by silver wire thrust through the nose from side to side. These clamps were retained in place several weeks, and when removed the nose seemed much improved; but after a few weeks it flattened down again, and it became apparent that nothing had been gained.

Remarks on the paper by Drs. Buck and Detmold.

Dr. Buck then described a new method of reconstructing the lower lip, illustrating the operation of the cadaver, and exhibiting a patient. Drs. Detmold and Hamilton approved of the method as highly original and ingenious.

The Academy then adjourned.

STATED MEETING, MARCH 4, 1869. DR. H. D. BULKLEY, PRESIDENT, IN THE CHAIR.

DR. GUSTAVUS S. WINSTON was elected a Resident Fellow of the Academy.

Names of delegates appointed by the Council to represent the Academy at the American Medical Association were read. Some specimens of pepsin were presented to the notice of the Academy by the President, and referred to a special committee.

The subject of noxious and contagious emanations was again discussed by Drs. E. Harris, S. Rogers, and others.

STATED MEETING OF MARCH, 1869. DR. H. D. BULKLEY, PRESIDENT, IN THE CHAIR.

DR. THOS. R. POOLEY was inaugurated a Resident Fellow.

A paper on the Physiological and Pathological Asymmetry of the Eye was read by Professor H. Knapp, M.D. (published elsewhere).

A vote of thanks was presented to Professor K. for his interesting paper.

DR. NOYES remarked, that while the Germans deserve the credit of patient investigation and originality in the cultivation of this branch of medical science, yet Americans had improved the

practical application of the knowledge thus gained. He called attention to the claims of Dr. John Green, of Boston, whose published diagrams are so valuable in detecting, in a practical way, the errors in vision in the making and selection of glasses.

The Academy then adjourned.

STATED MEETING, APRIL 1, 1869. DR. H. D. BULKLEY, PRESIDENT, IN THE CHAIR.

The President announced the death of Dr. Alexander H. Stevens, late President of the Academy, and, in compliance with the request of ten Fellows, called a special meeting for the purpose of paying tribute to his memory.

DR. J. C. DALTON exhibited with the microscope specimens of trichina spiralis, with accompanying remarks.

DR. GOUVERNEUR M. SMITH then read the paper of the evening, on the "Etiology of Bright's Disease, with Remarks on the Prophylaxis," for which thanks were awarded. (Published in Transactions.)

The Academy adjourned.

STATED MEETING, APRIL 15, 1869. DR. H. D. BULKLEY, PRESIDENT, IN THE CHAIR.

A paper was read by Dr. D. B. St. John Roosa, entitled, "A Statistical Report of 500 Cases of Aural Disease, with Brief Remarks." (Published elsewhere.)

Academy then adjourned.

STATED MEETING, MAY 6, 1869. DR. H. D. BULKLEY, PRESIDENT, IN THE CHAIR.

The President read to the Academy a report from the Committee of Award on the "O'Reilly prize" to the Council, assigning the prize to Dr. Robert T. Edes, of Boston Highlands, the successful competitor.

DRS. C. A. HUNT and HORACE CARUTHERS were elected Resident Fellows.

A paper was read by Dr. HANBURY SMITH, on "Mineral Waters as Therapeutic Agents," followed by remarks by Drs. Squibb, Peaslee, and Smith.

Society then adjourned.

STATED MEETING, MAY 20, 1869. DR. H. D. BULKLEY, PRESIDENT, IN THE CHAIR.

DR. BUCK exhibited some cases of rhinoplasty, detailing their history and the manner of operation in each case.

DR. W. C. ROBERTS then read a paper

ON IMPREGNATION, STERILITY, AND SUPERFŒTATION.

Mr. President and Gentlemen,—Before proceeding to the main subject of this paper, allow me a few supplementary remarks on the subject of menstruation, which I have already discussed before you.

The principal authorities on this subject are Négrier (1840, Rec. sur les Ovaires humains); Gendrin (Phil. Treatise on Med.); Pouchet, Bischoff and Raciborski, and Rouget, in 1858.

The ripest vesicle is that which periodically undergoes the changes and developments which lead to its opening, through which the ovule and débris of the granular membrane escape.

The clot which remains behind, and is in part absorbed, forms the corpus luteum.

One or more ova may maturate at the same time on each or either side. True corpora lutea are never seen in virgins, and it is universally admitted that coitus may hasten, though it does not determine, the fall of the ovum, or even induce more frequent ovulations.

But neither is the vesicle ruptured, nor the tube applied to the ovary, during the act of coition, but several days elapse before it arrives at sufficient maturity to discharge its contents, until which time the tube does not change its ordinary position (Haighton). Coition and ovulation are quite distinct from each other. Corpora lutea have been seen in the ovaries of girls who had never menstruated; and in such cases fecundation may take place, and has often been observed.

Again, menstruation may take place, and the ovum, arrived at a certain period of development, remain sometimes stationary and abort without breaking, in which case fecundation cannot occur.

Menstrual molimina, without hemorrhage, indicate abortive ovulation.

A comparison of 8,241 observations has induced Joulin to believe that in France, and other temperate climates, first menstruation is most frequent in the fifteenth year. The following statement is interesting:—A woman, according to Clos, noted the epochs of her menses for 27 years, or 293 menstruations. In this general total there were 2 intervals of 24 days; 13 of 25; 29 of 26; 52 of 27; 72 of 28; 36 of 29; 26 of 30; 8 of 31; 7 of 32. A woman who only menstruates two or three times a year (Joulin) has several times become pregnant during the intermenstrual period, and has two sisters in the same physiological conditions. Joulin mentions two circumstances, the first of which has often been told me: the simple change of linen, in some women, suffices to suspend, in others to reproduce, an arrested menstruation.

The vascular layer of the mucous membrane of the uterus, above the neck, denuded of epithelium, alone furnishes the menstrual blood.

Vicarious menstruation can only be considered as an inexplicable functional aberration of the nervous system. It neither troubles ovulation, nor prevents conception. It is not always *blood* that flows from elsewhere; sometimes only a periodical and obstinate gaping has been observed; sometimes diarrhœa; sometimes a milky flow; sometimes a tumefaction of the inner canthus of the eye, lasting some days; a serous flow from the ear; a periodical sweat, &c. Cases which seem perfectly reliable, cited by Cazeaux from Deventer, Perfect, and Churchill, show that ovulation can not only go on in pregnancy, but, in these cases, during pregnancy only, being absent at other times, which is inexplicable.

The cessation of menstruation depends on that of the development of the ovules, and the atrophy or obliteration of the tubes.

In women of ardent temperament, the "menopausis does not destroy the venereal appetite." I took care, says Joulin, of a lady completely deprived of vagina, no trace of uterus, who had never menstruated, who nevertheless sought the incomplete caresses of her husband, and felt acutely the genital sensations. Cases are cited by Bernstein and Blancard, in which menstruation continued for 80, 90, and 106 years. In reference to the probabilities of certain diseases—cancer and polypus—supervening at this period, I may just mention that statistics show that the former (cancer) most commonly begins at 20 to 35 years, and the latter (polypus) occurs, according to Malgaigne and Dupuytren, later still; of 51 cases observed by the former, the maximum was 30 to 40 years, and by the latter, in 57 cases, 19 at from 30 to 39, and 25 from 45 to 49.

So much for menstruation. Now, of fecundation. In order that fecundation may be effected, it is not necessary that the penis shall penetrate or enter the vagina, for in some exceptional cases, it is sufficient that the semen shall be emitted upon the vulva.

Husbands are very apt to employ some such precautions, and think themselves perfectly safe. Besides the case that Gooch relates, Joulin mentions another, whose large hymen was intact, and who had only had a single approach, rather perivulvular than vulvar. It is, then, in exceptional cases, sufficient that the semen should come in contact with the external organs of generation. Impregnation may be effected artificially. The desire for copulation is independent of puberty and ovulation; for Onanism is common in very young children, and erotism in those who have passed the cessation of the menstrual period. Some very fecund women never feel pleasure in coitus; many are never fecundated, although very erotic, or only after many years of married life, or only once, or by one or other husband, or at long intervals. Animals, who only cohabit when in heat, almost always conceive. The ridiculous ideas of the ancients on the subject of fecundation are now thoroughly abandoned, and the basis of the modern doctrine of fecundation unanimously adopted is, the secretion of the ovum by the ovary, and, when ripe, its fecundation by the

semen of the male, when living: this secretion taking place during menstruation, after menstruation, without menstruation, seldom during the complete interval between menstruation and menstruation, but sometimes under such circumstances, as to render it probable that coition had hastened the detachment of the ovum.

The semen is a complex fluid, but its most important constituent parts are animalcules called spermatic cells and zoosperms, spermatozoa, or zoids, according to the idea which is formed of their nature. These latter have a head and tail, and a long body, and move across the field of the microscope with varying activity. Obstructed by an obstacle they make sustained efforts to extricate themselves, and then rapidly flee, as if to escape danger. Sometimes they turn from it upon themselves, and choose another route. Seemingly, their volition is directed by instinct; they pass the space intervening between the os uteri and the ovaries pretty rapidly, directed as it were by instinct towards the latter—the ovum which they are to impregnate. In a natural temperature they live for 48 or 72 hours even, and are found living in the cadaver after 24 hours. In bitches, they have been seen to move 7 or 8 days after copulation. The menstrual blood animates their movements, which become irregular; they displace the blood globules, and make a way for themselves through them. Alkaline uterine secretions, leucorrhœal fluid, acids, vaginal mucus, and urine destroy them, and so do electricity, narcotics, and strychnine. All this is evidence, according to Joulin, of their *animal* character. They are symmetrical, and formed on each side of a medium plane. Probably they are reproduced, but they grow, and are certainly nourished. Strange beings which, by union with an ovule, are capable of communicating to it not only the physical resemblance, but the temperament and constitution of the parent.

These animalcules spring from an egg, which is developed in the fluids of the spermatic ducts, capable, the male ones, of segmentation, within them, into male embryonary cells, analogous to the yolk of the ovarian egg. The granulations which they contain condense themselves upon a point of their periphery, and form a nucleus which will be the head of the spermatozoid; then the tail proceeds, winding around the edge of the embryonary cell, which finally bursts, and the spermatozoid is free, with others, inside of the common male cell. They lie in parallel groups, heads together, and tails in a curved bundle.

Lastly, the walls of the male ovule give way, and the animalcules are set at liberty, and detach themselves from each other. Appearing at puberty, they are found afterwards at all periods of life, and in men of 82 have been found as numerous as in the adult. The semen of animals contains spermatozoa only in the rutting season. The matured ovum may, according to circumstances more or less favorable, burst at any period during menstruation.

It has been found broken on the 1st day, and intact, although distended, on the 5th.

When once the ovule has quitted the ovary, it becomes entirely free, and, under the mysterious impulse of nature, follows its onward and downward course as surely as a being who moves and reasons.

The material contact of the semen and the ovule, both animated by vitality and perfect in themselves, is the essential condition of fecundation; and the intimate fusion of these two elements is alone capable of giving birth to the new being.

Complete coitus, we have seen, is not indispensable, and the erotic spasm on the part of the woman plays no part in fecundation; but if any obstacle impedes the immediate contact of the two germs, conception is impossible. The exhalations of the semen alone do not suffice. Filtered semen has failed to effect it. No part of the semen but the animalcule suffices. That of the male frog is divided into a liquid portion, contained in a pouch situated at the extremity of each deferent canal, and a solid part, contained in the testicle, and wholly composed of spermatozoa. The former, according to Coste's experiments, was constantly unfertilizing, while the other vivified the eggs and produced the tadpole. The fecundating power of the animalcules seems connected with their motility, for it diminishes and is completely extinguished with their movements. Semen without living animalcules cannot fecundate. They act as a sort of ferment. Now how do the animalcules get to the ovum? Has the vitelline membrane any opening in it to admit them? Nevertheless they have been seen within it, and in immediate contact with the yolk, where they part company and disappear by liquefaction. The fact is certain, although the passage cannot be detected.

The quantity of semen necessary to produce fecundation is exceedingly small. The merest drop of a high dilution, applied to the eggs of a female frog, suffices to impregnate them; but there must be more animalcules than ova. 225 spermatozoa, in the hands of Prevost and Dumas, impregnated 60 eggs in 300.

Where does the fecundation take place? In the ovary (and if so, in, or on the ovum), as is certainly true in some animals, in which case the animalcule must pass through all its investing envelopes; *on* the ovary, or in the first portion of the tube? The two last are generally admitted. From the fact that when the ovum is in the tube or uterus, it has no covering, I should infer that the animalcule must pass within the ovarian coat, upon or within the vitelline membrane.

The action of the vibratile cilia of the uterus and tubes, being from within outwards, in the direction of expulsion, cannot account for the progression of the semen; and it is too slow for any aspiratory spasm, so that the spermatozoa must wriggle their own way. When the semen is deposited in the vagina (not sent into the uterus), or even about the vulva,

20 or 30 minutes are required to enable them to enter the uterus. Capillarity, which operates upon no other fluids injected into, or contained within the uterus, cannot account for it. The motility of the spermatozoa, and their power of overcoming obstacles, alone suffice for this purpose. The ovum, indeed, owes its progression to a mechanical agent, the contractions of the tube and the action of the vibratile cilia, which is directly opposed to the course of the spermatozoa: the passage from the uterus of the spermatozoa into the tubes occupies from 8 to 12 hours. Arrived at the free extremity of the tube, they reach to, or upon the ovary, by means of the fimbriæ which unite the *pavillon* to this organ. If within, or upon it, they find a ripe ovum, fecundation may result. The tubes take two to six days to transmit the ovum to the uterus, where, if previously fecundated, or, when fecundated there, it stops and is developed, imbedded in decidua. If not, it escapes within 10 or 12 days with the decidua. The period most favorable for impregnation is that of menstruation, or at least the few days which precede or follow it; but the difficulty is to account for fecundation during the days intermediary to this period. Fecundation and coition are separated for at least the time it takes for the spermatozoa to pass through the uterus and tubes, and reveals itself by no special signs. No law seems to govern the procreation of the sexes. Women deprived of one ovary (as men wanting one testicle) have children of either sex; or they are conceived in either ovary. Sterility is the inaptitude to fecundation, on the part either of the male or female. Impotence, the impossibility to accomplish copulation in the part of the male. Men may be incapable of fecundating, however, who are capable of entire, or, as in the case of eunuchs, of incomplete copulation; for the latter have sometimes an incomplete power of erection, although, as they lack the stimulus to venereal appetite, it is gradually lost after a time. I do not know whether there are any spermatozoa, dead or living, in the semen of eunuchs, and after a time, I suppose, the secretion ceases. This may depend either on an absence of ejaculation, or the ejaculation of a fluid wholly deprived of spermatozoids (or at least of living spermatozoids). The one is called aspermatism; the other, aspermatozia.

Sometimes aspermatism is dependent on a simple atony of the vesiculæ seminales and ejaculatory ducts. It is sometimes purely nervous: there is no erection, however much desired or solicited: or there may be effusion without erection. This often results from debility, and is to be overcome by time or treatment. It is common among old men. Anorchids or cryptorchids are sterile. The latter have strong erotic propensities, but their semen is absolutely devoid of spermatozoa: whether they have erections I do not know. A single testis suffices to fecundate. Anything which obstructs the deferent canal causes sterility. Mules always, and hybrids generally, are sterile, except in cases of close affinity of

race, as in the dog and wolf. The crosses of these products soon become sterile; not so with man. His hybrids are always fecund throughout their descent, a proof of the unity of the human race.

Whenever the spermatozoa contained in semen are completely *motionless* the man is sterile. This can be ascertained only by the use of the microscope. If the husband be perfect, and living spermatozoa enter the cervical canal, and no fecundation follows conception, the fault must lie with the woman and on the ovum itself.

Sterility in women is more obscure, and the elements of diagnosis often escape our investigation. A too alkaline uterine secretion destroys the vitality of the spermatozoa more readily than acids in the vagina, though this is also injurious. The former often depends upon inflammation, which must be remedied. Dr. Gilman told me of a lady who for a long time preserved herself from impregnation by injecting, immediately after each coitus, water acidulated with sulp. acid; and I have no doubt that urine would do as well, although, curiously enough, women conceive with vesico-vaginal fistula. Absence of ovaries and uterus, and obliteration of the vagina, are mechanical obstacles to impregnation,—the former irremediable; the latter, if complete and congenital, allows the menstrual fluid to accumulate and be retained behind it; or it may result from inflammation. A very large and resistent hymen is unfavorable to impregnation; but fecundation has occurred where the passage has been scarcely perceptible, and its division at the time of labor has been necessary. It would be interesting to know whether and in what manner women menstruate under these circumstances: easily or with difficulty? Spasmodic contraction of the vagina (vaginismus) causes so much pain as to prevent coitus, and it must be dilated or operated on, although it cannot be considered as a necessary obstacle to fecundation.

Upon the subject which follows, the remarks of Joulin are so interesting and practical that I shall condense them. He says: The contraction which has its seat at the internal orifice of the uterine neck is one of the most common causes of infecundity, and particularly among nulliparæ. But women who have had children sometimes exhibit this disposition, which oftenest coincides with an extreme narrowness of the cervical neck and imbrication of the folds of the arbor vitæ. This cause of sterility, which is usually accompanied by the phenomena of dysmenorrhœa, was until lately unknown. The treatment consists in dilating the constricted region. I prefer a cylinder of prepared sponge, small to begin with. I have obtained two successes with this proceeding, Becquerel four, and Meisteler seven out of nine. In the first application, the sponge does not pass the internal orifice, it simply dilates the neck; but later, that orifice becomes permeable. The sponge applied swells immediately, and the woman can attend to her business without feeling the least inconvenience. If not removed, generally about the third day a discharge takes place of a clear, abundant, and fetid fluid, which disappears on its removal.

I renew these applications twice a month, ten days before and after menstruation. When the dilatation is sufficient, I suspend their introduction, to recommence again if the stricture tends to reappear; for often amelioration is only temporary. It supplants the incision, which, unless the sponge tents are used simultaneously, cicatrizes and contracts, leaving things as they were. It is simpler and less dangerous, and women, at least if they have not patience to follow out the plan, are often relieved of their dysmenorrhœa.

Uterine deviations are not causes of sterility, for it is not necessary in copulation that the extremity of the penis shall be in direct relation with the os uteri. It passes behind into the utero-vaginal cul-de-sac, and the semen is deposited in the vaginal cavity, not projected into the os. Shocks are thus avoided, which from the close proximity of the os to the vulva, and the great length of the penis in erection, would be horribly painful, if the organs came into direct contact, as happens in cases of anteversion. Unless, then, when very intense, and the angle very great, displacements of the uterus do not offer any serious obstacles to conception.

Impermeable tubes, of both sides at least, is an absolute cause of sterility, and the diagnosis is impossible. It seems to me that in the case of double obstruction there could be neither ovulation nor menstruation; or at least no outward and visible sign; and the ovum, if disengaged from the surface, must fall into and be lost in the abdominal cavity. Disease of both ovaries and atrophy are certain causes. The temporary or prolonged absence of the menses shows no lesion of secretion; for women often conceive under these circumstances. But what is strange, and not alluded to, is that a man may enjoy all his powers, and his wife menstruate regularly and be well conformed, and yet never procreate, or only once, or after long intervals of married life, or after many years of antecedent pregnancy; or a woman sterile by one man may conceive by another. This is not so difficult to explain; but the other circumstances afford wider scope for conjecture.

It has been said that a woman born a twin with a male is sterile; but facts and statistics show that the idea is fallacious. The mean of fecundity of marriages in Europe is 4·2 per union.

It has been seen that semen of proper character must encounter a ripe Graafian vesicle, either in the ovary, on the surface of the ovary, after its dehiscence, and when denuded of its ovarian coverings: in some portion of the tubes or in the uterus. It is not easy to understand how the spermatozoon can pass through the ovarial coverings, and Pouchet has been inclined to deny its possibility, saying that there was no true ovarian pregnancy; but facts are, I think, opposed to this theory; although, in many cases of the kind, when the ovum had not quitted the ovary, a rent had occurred, which may have been a prolongation of the rupture by which the ovule had in part escaped through the ovarial coverings and given admission to the spermatozoa. But whether a well-authenticated

case exists, in which fecundation has occurred in an ovule entirely within its ovarian capsule, I am unable to say. As to cases in which the ovule has escaped, and still hangs to the surface of the ovary by some of its adherent tissues, there is no doubt; and although any opening in that of the human female is invisible, it is certain that spermatozoa have been seen within it, and probably always do enter it; but how they effect in it the subsequent changes and phases of development which the ovum undergoes, no one, so far as I have seen, has attempted to conjecture. The animalcule traverses the vitelline membrane and dissolves itself in the yolk. If the dehiscence of the ovule, or the ovary, corresponds with the first appearance of menstruation, and the ovule is to be fecundated on the ovary, it may be so as soon as the spermatozoa reach it: and as they travel quick, and their motions are animated by the menstrual blood, it may be impregnated on the ovary in the course of eight or twelve hours, if coition takes place during menstruation, and women may be fecundated while menstruating. But this is certainly very seldom the case among us, and it is not until some day or two after menstruation ceases that women allow the approaches of their husbands,—sometimes not for ten days or more. By this time the ovule has probably passed into the tubes, or through them into the uterus; and therefore, I presume, it is oftenest in this organ that impregnation takes place when the coitus occurs after menstruation, although it is possible that spermatozoa emitted by a previous coition may have ascended to the ovary, and retaining their vitality, as they do for an unknown period, may reach the ovule in some part of its course, and cause it to come already fecundated into the uterine cavity. Then again, as the ovum retains its vitality for an unknown period of its discharge, and cannot be fecundated without coition, there is no telling how long it may be after it, nor in what part of its course the fecundation may occur. The exact length of time, says Dalton, preceding and following menstruation, during which impregnation is still possible, has not been ascertained. It varies more or less in different individuals. Impregnation, says Joulin, does not take place at the moment of coitus, but in at least twelve hours afterwards; and if the spermatozoa have been found living eight days after their migration into the female organs, this time at least may elapse after coitus, whether before, during, or after menstruation. After this comes an intermenstrual period, in which, if the ovary emits no ripened ovum, no spermatozoa can reach it or impregnate it; and yet it is a curious fact that a woman can be impregnated at any moment, and are impregnated at all periods within the month, whether the menses are existing, imminent, just past, or not again immediately or soon expected: a fact which gives opportunity for curious physical speculation. I do not by any means feel so sure as Pouchet does, about the perfect immunity of the intermenstrual period, nor how to explain the occurrence. Perhaps there is such a thing

as an ovum, quite or nearly ripe, being detached by the excitement of coitus.

It cannot be told how long ovular transit goes on after menstruation ceases; but it is very certain that after it ceases, and the ovum has become decomposed and passed away with the uterine secretions, impregnation is impossible until a new one has taken its place in some part of its course between the ovary and uterus, or in the uterus itself.

Pouchet says that "generally there is one ovum, which escapes either immediately after the cessation of the menses, or in from one to four days after. Now here nothing is said as to the escape of an ovum *during menstruation;* and if so, there could be no fecundation; but the ovarian vesicle has been found broken on the first day. I do not believe that there is any period during menstruation at which an ovum may not be detached, seized by the fringed extremity of the Fallopian tube, and carried into it.

But if it be, as Pouchet says, after the cessation of menstruation, then, according to him, it is either immediately after, or one, two, three, or even four days later. The tubes take two to six days to transport the ovum into the uterus, so that we have a period of at least ten days before the ovum gets into the uterus, and during which it may be fecundated, there or elsewhere, in its course. If it meet with semen in its course, and is fecundated, it remains in the uterus and is developed; if not, it escapes with the decidua in ten or twelve days. This, he says, is the only period in which women conceive, and the remaining time between this and the next menstruation is the true intermenstrual period, during which impregnation is physically impossible. Avrard says that conception can occur up to the fourteenth day, counting from the commencement of menstruation, and is after this time impossible. Sometimes, however, the menses, or, at least, a flow of blood, reappears in the course of a few days after the catamenia have ceased, and before the next should recur. Is this only a hemorrhage, or a menstruation attended with ovulation, and again subjecting the woman to the possibility of impregnation, and accounting for the fact of the possibility and frequency of intermenstrual impregnation, and for some of the uncertainty which women labor under in calculating the date of their pregnancy?

EXTRA-UTERINE PREGNANCY IS OF THREE KINDS,

Ovarian, tubal, and abdominal. Pouchet denies that there ever exists a true ovarian intra-capsular pregnancy, and, as before said, although it may not be wholly denied, it is certainly very difficult to understand how a spermatozoon can enter and impregnate an ovum within its own proper coverings. Burns (p. 164) tells us of a fœtus of the ordinary size at six months found in the ovarium, which had given way; Boehmer, of

an ovarium ruptured and fœtus half expelled. Now these rents may have been only enlargements of the original rupture of the ovarian coats at the period of ovulation, and the ovum may not have escaped from the ovarium and have been there fecundated. Pouchet says that what has been called ovarian pregnancy does occur. The sac has been ruptured, and the ovum protruded, and fecundation takes place on the outside. In such a case the ovum, not being properly embraced by the fimbriæ of the tube, may fall into the peritoneal cavity, and, either proving fatal from hemorrhage, or subsequently contracting adhesions, being, of course, previously fecundated, become abdominal; and in spite of the apparent impossibility and great rarity of such an event, cases on record seem to show that it has really occurred. In Granville's case, the inferior and left half of the left ovarium presented a surface consisting, at two or three points, of diaphanous membrane, through which the fœtus was seen. The tunics, being successively distended and thinned, finally burst, and thus *the fœtal* coverings partially protruded into the abdominal canal. This does certainly seem to show, that however the spermatozoa enter it, an ovum may be impregnated within the ovarium, and within its ovarial coverings.

The late Mr. Moore, says James, showed me a preparation taken from a woman in the sixth month of extra-uterine pregnancy. The fœtus, of ordinary size at that period, was found on the ovarium, which had given way, and blood was effused into the abdominal cavity. Laroque: Ovarium found ruptured, and abdomen full of blood. Forrester (Ann. of Med., vol. ii., 379): fœtus found *in the ovarium*. Hair, bones, etc., are often found in the ovarium.

Tubal pregnancies are not extraordinary, because the ovum in its progress through them *may meet* with semen, be impregnated, and if delayed or obstructed in its passage towards the uterus, be developed there, as often has happened. And I do not see the necessity for supposing any antiperistaltic action of the tubes, by fright or otherwise. A good many ova, probably, do fall into the peritoneal cavity; but not being fecundated they perish, are decomposed, or absorbed.

Another interesting question arises as to ova which ripen, burst their envelopes, and are not attended with a menstrual discharge. Do they wither in situ, or escape through the uterus and vagina when not fecundated?

Superfœtation

may be of three kinds. 1st, Arising from double uterus. 2d, The result of two successive and quickly succeeding coitions, perhaps by different fathers, impregnating thus, within a few hours, or days, two successively matured ova; or by a single coitus, the spermatozoa maintaining their vitality sufficiently long to impregnate the second ovum; and 3d, Or two children may be born, in the

same labor, with a certain interval between each, which are of different degrees of development—one fœtus having compressed and interrupted the development of the former—twin births, the result of one impregnation. The cases most difficult of explanation are those when, as in the case of M. A. B. a lively child was born on April 30, and on September 17 (four months and a half) a second apparently mature child was born: the uterus, p. m. being found single. It is difficult to conceive how, as a result from subsequent impregnation, the ovum finds its way into an already occupied uterine cavity, lodges and grows there.

Pouchet seems to think that in these cases the development of the second child, temporarily arrested, goes on after the birth of the first. Churchill frankly admits that the doctrine of superfœtation being insufficient, no theory as yet advanced is adequate to their explanation.

James says that when two or more ova are on their way to the uterus, and one had entered it and gone on to a certain extent, a second may be excited into action and brought down in its turn, and both fecundated by one coitus, or the second by a later one, which could account, in some measure, for the delay which is known to occur in the delivery of the two. Two or more ova are certainly often ripe at the same time, and often detached and transmitted at or about the same time, or there could be no dual, triple, or quartal births, and in several recorded cases, although only one or other ovary was perforated, in both the ovaries were large, vesicles distended, and both Fallopian tubes turgid and red. It certainly does surprise me that, under the circumstances, such multiple gestations are so rare.

In certain animals which bring forth many at a litter, both ovaries certainly contribute to the common stock.

Then again, not uncommonly the fœtus is developed partly in the ovarium and partly in the tube—ovario-tubal pregnancy—and these cases are, I suspect, often mistaken for ventral or interstitial pregnancies. This, I infer, is what Dr. Rogers thinks of them, in an elaborate paper in the Transactions of the American Medical Association, advocating a surgical treatment of this generally fatal condition when death is imminent from rupture of the sac and hemorrhage. The ovum never passes fairly into the tube, not being properly embraced by its fringed extremity, grows downward into the abdomen, the fœtal movements are felt in the tumor, adhesions are contracted, the placenta sometimes adhering to the spine, kidney, stomach, intestines, broad ligaments, fundus uteri, &c., or, together with the fœtus, contained in a sac. Now, I say again, some of these cases may be cases of ruptured uterus; some of ventral pregnancy, if there is such a thing; and some of ovario-tubal pregnancy. In some cases the result is, sooner or later, fatal. In others the fœtus is retained for a shorter or longer time—even 50 years and upwards—undergoing certain changes, ossification, calcareous deposit, &c., the woman, in the interim, con-

ceiving again naturally, or again otherwise; and sometimes gotten rid of in various ways, by incision, abscess through the abdominal walls, by the anus, bladder, vagina; sometimes ending favorably and sometimes in death. Another species of pregnancy is called interstitial, when the ovum is detained in an interspace of the fibres of the uterus. The case related by Churchill does not, to my mind, clearly establish its true place among extra-uterine fœtations, and I shall not dwell upon it.

From what I have before said on the subject, it is not quite clear whether in women, as in some animals, the fimbriæ totally envelop the ovary. If they did, I think ovario-tubal pregnancy would be more rare. Rouget says of the mechanism of the adaptation of the oviduct to the ovary, at the moment of the escape of the ovum, that the direction of the two orders of muscular fibres in the ovarian and round ligaments, which embrace the whole length of the tube and pavillon, explain the movements executed by these organs to carry themselves backward and inwards, and the possibility of the tube inflecting on itself and applying the pavillon to the surface of the ovary, like the action of purse strings. The fimbriæ in some sort, lick the surface of the ovary, and the peristaltic action of the tube receives the ovum into the pavillon, and, with the cilia, transports it into the uterus, or else they have some means of attracting the ovum into the tube. This is, of course, oftenest on the side of the ovary which has furnished the ovum; but there are said to be instances of ovular migration from opposite sides.

To sum up the subject of superfœtation, let me say that *it*, that is to say, conception, in a woman whose organism contains already a fecundated ovum, is possible only under the following conditions:—

1st, When the two fecundations take place only at a few days' distance, and before the first ovum has reached the uterine cavity, which accounts for gemellar pregnancies giving birth to products of two different races. 2d, In cases of bicornute uteri. 3d, When a woman, carrying an extra-uterine fœtus, is anew fecundated, in which case long intervals may occur. Other cases cited, not referrible to either of these heads, are (for the most part) instances of double pregnancies whose products, unequally developed, but conceived about the same time, are expelled at different periods.

A few words more on the subject of *Gemellar Pregnancies.*

In 87 accouchements there were 1 double pregnancy—3 in 7, 679. Four are met with in proportions which it is impossible to indicate. Cazeaux cites instances of 5, and Ramsbotham one of 15!

Some are due to the dehiscence and simultaneous fecundation of one or more ova, in one or both ovaries, either by one coitus, or a second at a few days' interval. Hence the possibility of the simultaneous birth of a white, a negro, or a mulatto. 2d, A single vesicle of De Graaf may contain two or three ovules. Bischoff has seen in a woman two ovules contained in the same vesicle

still intact. At the moment of its rupture each ovum becomes distinct, and is separately fecundated. Or two vesicles of De Graaf may fuse, and their products become contained in one cavity; or an ovum may contain two germinative vesicles, and two vitelli, and from the fusion of the primary elements, result double monsters; or two embryos, originally distinct, may fuse in a single amnios, and if fecundated by parents of a different race, the anomaly reported by Prus, of a bicephalous fœtus having one head white and the other black, may result.

STATED MEETING, JUNE 3, 1869. DR. H. D. BULKLEY, PRESIDENT, IN THE CHAIR.

DRS. MARCUS P. STEPHENSON, WM. B. LEWIS, and I. I. SPRENG, were elected Resident Fellows.

DR. BUCK exhibited two cases of rhinoplasty, with accompanying remarks.

The paper of the evening was then read by Dr. Jas. Kammerer, on "The Pathological Conditions Causing Sterility in the Female." (Published in the Transactions.)

STATED MEETING, JUNE 17, 1869. DR. H. D. BULKLEY, PRESIDENT, IN THE CHAIR.

DR. VAN KLEEK announced the death of Dr. Jacob T. Gilford, one of the earliest members of the Academy, well known as an experienced and successful practitioner and a strict adherent to professional courtesy. After which, Dr. Stephen Rogers read a paper on

SUMMER MORTALITY IN THE CITIES OF THE UNITED STATES.

The interest and the importance which attach to the subject we have selected for this paper, is, perhaps, best shown by a tabular statement of mortality rates, by seasons, in some of the large cities of the world, as compared with our own.

Quarter ending	London.	Paris.	New York.
June 30.....	15,630	10,875	5,540
Sept. 30.....	16,406	10,520	8,660
Dec. 31.....	18,413	18,000	4,760
March 31.....	20,422	11,850	5,900

It will be observed that while in the two great cities of Europe the mortality runs highest during the cold months, in our own city the most fatal months are the ones including the heat of summer.

One of the most striking examples of the destructive influence

of atmospheric heat upon large masses of population, was furnished during the summer of 1868 in this city. Under a mean increase of the temperature of 8°, during the week ending July 18th, the mortality of this city rose from 600 to 1100. True it is, that, under similar circumstances, the degree of humidity of the atmosphere has its effect upon health, but to nothing like the extent which characterizes the thermal state. It is generally understood as true, for example, that a dryness of the atmosphere counteracts heat, and that, on the contrary, its humidity augments the influence of heat. But while that is no doubt true in principle, the practical results of the fact are very liable to deceive us, as is shown in the instance just cited, when the mortality almost doubled in a single week under an increase of 8° degrees of temperature, though the humidity was $\frac{80}{1000}$ less than during the previous week. Its effect to diminish mortality was therefore greatly outweighed by a few degrees of heat. Dr. Ballard's first aphorism is then unquestionably founded in truth. It runs as follows: "An increase of atmospheric temperature is normally associated with an increase of general sickness." Its illustration is not apt to be very marked in this part of our country, except the increase be above 75° or 80°. That is to say, when the temperature, having been about 75° for a time, suddenly runs up to 78° or 80°, the effect on public health is at once apparent; and still more when it runs from 80° upwards. Changes, however, from 60° to less than 75° do not in this country produce results either so uniform or striking. Starting from these high points, the third aphorism of this author is equally true; viz.: "That for the most part, the increase or decrease of sickness is proportional in amount to the extent to which the atmospheric temperature rises or falls." Results however are liable to be prolonged, so that what is effected by a sudden rise of temperature this week, often does not reach the record till the next week, when by a fall in temperature the general health is much improved. Hence improved weather tells less accurately in the death lists than do sudden accessions of heat. It will no doubt be admitted without further statistical reference, that the atmospheric heat prevalent during our summers is the more or less direct cause of the fearful death rate we have to annually record. The most generally recognized method of its destruction of human life is by the pathological effect known as sunstroke. A very just conception of the amount of life destroyed by atmospheric heat directly, may be formed by inspecting the following table:—

Deaths attributed to sunstroke in New York.

In 1863	133.
" 1864	37.
" 1865	11.
" 1866	310, and effects of heat.
" 1867	4.
" 1868	171, and effects of heat.

It will be observed that though 1868 was a year of almost unprecedented heat, its mortality from sunstroke, and recognized effects of heat, did not equal 1866. This finds a very instructive explanation in the fact that, during the intense heat of that season, a large number of mechanics and laborers, who furnish material for this cause of death, were engaged in a strike, and consequently not exposed as during labor.

As to the exact pathology of the cause of death in the condition known by the various names sunstroke, coup-de-soleil, coup de chaleur, ictus solis, insolatio, erethismus tropicus, &c. we will here venture to simply say that the great bulk of proof stands in support of the view that it is one of more or less suddenly depressed vitality. The symptoms, however, are so unusual for conditions of depression or exhaustion, that the profession was very slow in learning the real truth in respect to it. From a somewhat careful perusal of the history of sunstroke, it has appeared to us that more has been learned of its pathology and treatment by copying the instinctive, or perhaps experimental practice of unenlightened nations, than by any other means. The poor Hindoo taught the learned and proud Briton that the means by which life could be saved, in *threatened* or actual sunstroke, was simply to keep the surface of the body wet and cool, and to place the patient in a state of physiological and physical rest. And that is about all we know of the matter to-day. This simple rule is applicable to the two forms of this diseased condition well known as febrile or hot-skin form, and the syncopal form with cool skin and depressed pulse. In the latter form, the skin being already cool, there is of course no indication for the water to the surface, but the rest is indispensable.

The fatal cases of sunstroke, however, are very rarely of this form, but of the ardent, febrile, and often apoplectic form. The same semi-barbarous Hindoo also taught that the condition could generally be prevented by the same care not to produce fatigue, and whenever the natural moisture of the skin ceased to keep it cool by evaporation, it was to be effected by moistening the surface with cool water, allowing it to evaporate, and thus protract the cooling effect. These affusions he practised, and still practises, upon the uncovered skin, occasionally varying it, as a matter of economy of time, by wetting the light cotton or linen clothing worn by the person. The theoretical and the practical result sought in this process is the reduction of the temperature of the blood, from which danger to life proceeds. Dr. Dowler, of New Orleans, taught the doctrine that there is as much danger perhaps from the heat conducted from an exposed sun-heated surface to the blood, as there is from that generated in the system itself by the chemico-vital processes going on. Hence he thought that the covering of the body with articles of clothing of considerable thickness was a valuable means of protection against sunstroke. The Hindoo, however, understands this to be an error,

as the slightest reflection must convince any one that it is.* It is a philosophical axiom that evaporation is more rapid, and consequently more caloric is consumed, from uncovered surfaces. Therefore the evaporation from, and the cooling of, an uncovered skin, bathed in perspiration, is much more rapid and effective than when covered. It is an easy fact to demonstrate, and one which we have often done, that the body bathed in perspiration is much cooler in the rays of the summer sun when uncovered, than when clothed in the usual style. It therefore follows that the nearer naked the human body can be allowed by the laws of society, the better state it will be in to resist the influences which induce sunstroke, and then, so long as perspiration is copious, there is little danger of sunstroke. But the arrest of the perspiratory process from nervous exhaustion, is the danger we have to look for and avoid, by supplying an artificial perspiration, under circumstances best calculated to cool the greatest amount of surface. As any means of cooling the blood under the circumstances attending sunstroke are generally beneficial, we will mention the invaluable one of taking ice-water. Nothing is so grateful and so useful to

* The idea advocated by one of the gentlemen who participated in the discussion had upon this paper, that the phenomena of sunstroke are to be accounted for by the sudden suspension of the process of elimination of effete and poisonous material by the skin, is a totally untenable one, for the following reasons:—First, Cases of sunstroke are not always preceded by a drying up of the skin; and hence in such cases there is no *even probable* reason for supposing that elimination of poison from the blood has been suspended, so far as the skin is concerned. Second, The time between the drying of the skin and the occurrence of the grave symptoms of the disease is, in a great proportion of cases, too short to render it at all probable that arrested elimination is the cause of those symptoms. Such rapidity has no analogue in the known suspension of the eliminating function of any organ of the body, if we except the lungs. Third, Physiological experiments upon the lower animals, and thermometric observations upon the human victims of sunstroke, suffice to show that the overheating of the blood and tissues of the body is the real cause of the phenomena of this affection. The argument, therefore, that to prevent sunstroke perspiration must be promoted by heavily clothing the person, even in tropical temperatures, has no pathological support, and is directly contradicted by the most extended observation, as well as by the teachings of natural history. If it were true, as this argument assumes, that the animal body is protected from solar and atmospheric heat by covering it, is it probable that the Creator would have made it a characteristic of tropical animals that they have little or no hairy covering? And in the temperate zone, would He have ordered that the thick and heavy fur and hair covering possessed by its animals in winter should fall off at the approach of summer, leaving the skin comparatively nearly naked during the heated months? We think no reflecting person can long entertain such views of the pathology of sunstroke, nor of the influence of heavy clothing in protecting from it. The example given, on the occasion of the discussion, of the heavy dress of the inhabitants of the steppes of Russia, has certainly very little weight in the argument; for while these people have but comparatively few hot days in the year, they occur during a period in which the atmosphere is extremely dry, so that evaporation of the perspiration and consequent cooling is rapid, even though the body be thickly covered.

the heated thirsty laborer or soldier as an unlimited supply of ice-water. The theoretical objection to large draughts of cold water after prolonged abstinence, and with devouring thirst, we have never seen a demonstration of; but, granting the objection has foundation under such circumstances, there is certainly no danger in cases where no deprivation has been practised, but, on the contrary, frequent and more or less copious draughts have been indulged in. This practice not only introduces a large amount of liquid to which caloric is imparted, thereby helping to keep down the temperature of the blood, but it supplies abundant fluid to be employed in perspiration, and thus serves to protect the economy more or less perfectly. These propositions being admitted, which we think must be, it follows that an overheating of the blood under high temperatures is normally prevented by the application of the cooling process to the capillary circulation; and upon this it would follow that the greater the amount of capillary circulation thus exposed, the more rapid would be the consequent depression of the mass of circulating fluid. This fact is too often ignored in both the prophylactic and curative treatment of the effects of solar heat, an example of which we have in the following published instructions:—

"1. When exposed to excessive heat, the natural perspiration or sweating must not be checked. Let the sweat flow, and do not throw off all the flannel garments.

"2. Wash the head and neck frequently in cold water, and, if exposed in the sun or in any very hot place, wear upon the top of the head, under a hat or otherwise, a light handkerchief or other thin folds of cloth wet with cold water. Remember that the head and neck must be kept cool, and also be free from the pressure of tight clothing.

"3. If headache, dizziness, a feeling of tightness across the forehead, a failing of sight, or a feeling of weakness and prostration ("giving out") occurs, let the person immediately go to a cool place and lie down, with the back and the feet covered, and have a few quarts of cold water poured slowly upon the head and the sides of the neck. If the symptoms are not at once relieved, send for the nearest good physician.

"4. What to do until the physician comes. If the patient is still sweating, let him drink freely of cold black tea or coffee; tea is best. But if sweating has ceased there is very great danger. Then the head, face, neck, and entire chest should be rapidly sponged with ice water, and a bag of powdered ice, well covered with a towel, should be kept under the head and the back of the neck; give a mixture of the carbonate of ammonia (hartshorn) and muriate of ammonia, eight or ten grains of each in water, every ten or fifteen minutes, until the physician comes, or until the patient rallies and sweats." *

The blood is quite as rapidly cooled by applications to a single

* Board of Health publications in the daily papers.

arm as to the head and neck, so that if all the extremities are included, the mass of circulating fluid is reduced in temperature in a fraction of the time required for applications to the head and neck only. It is a serious delusion to suppose that the blood temperature is materially reduced by the ice pillow. It is a fact well known to physiologists, that circulation is nearly perfectly arrested in capillaries whose temperature is reduced so low as 32° to 36°. Hence no important amount of cold blood leaves the tissues subjected to the influence of the ice pillow, to appropriate caloric from the highly heated fluid. The application of ice to the head and neck is therefore of very limited efficacy, and really prevents what it is supposed to accomplish. Water between 40° and 50° is far more efficacious in cooling the blood when applied externally; and when applied to the mucous membrane, from which there is no evaporation to prolong its influence, it may be used below 40°. Concisely, then, much of the mortality resulting from sunstroke may be prevented by the general adoption of the principles here sketched, thereby promoting free perspiration when it may be necessary for cooling the blood, and removing all possible obstruction to its evaporation from the surface, adding to its effect the free internal use of ice-water, and, when indicated, the free and general use of cold water externally, together with physical and physiological rest.

The ignorance of the laws of natural philosophy and pathology, displayed in this first rule, is not only characteristic of its author, but it echoes a deep-rooted error entertained by both the profession and the populace. The error consists in the supposition that the "flow" of perspiration is necessary to safety; that, no matter what becomes of it, if it simply flows life is secure. A more unfounded doctrine never was promulgated, and a more dangerous one could hardly exist. It has sweated many an unfortunate man to death. It, in the first place, is not true that a flow of perspiration is of itself a source of security in high atmospheric temperature. It is of no service against heat and its consequences, except by the reduction of the temperature of the blood effected by its evaporation. Perspiring is in itself an exhausting process, and therefore it is better to avoid it as much as possible by avoiding the necessity for it. This is done effectually by cooling the blood, which we have just seen may readily be done by the application of cool water to the surface of the body, by instituting the process of evaporation from the surface, by cool drinks, and by physical rest. We may also add, the application of cool air to the surface of the body. The quicker the perspiration is stopped, by transferring a man on the verge of sunstroke from an atmosphere of 120° to one of 60°, the sooner he will be placed in safety. It is a dangerous doctrine that a covering of flannel must be maintained during even our hottest days, as a protection against changes of weather that might arrest the flow of perspiration. What sane man of the most ordinary habits of observation, would venture to

say that he has ever seen other than the most salutary results, upon both individual and public health, from the reduction of the heat of the weather of 20° or 25°, say from 100° to 75°? Apply these facts, which cannot be controverted, to the prophylaxis and the treatment of sunstroke, and we shall have a just idea of the rule above quoted.

Any details of treatment and management need not be mentioned here, for they will suggest themselves to the mind of any physician. It is a thoroughly well understood fact that, other things being equal, the higher the standard of vitality maintained by the individual, the less liable he will be to require artificial aid to keep his functions going under protracted high temperature. At best, however, the vital forces are so taxed to support the refrigerating process, and their restoration is so much interfered with by disturbance during the usual hours of sleep, that most of the functions of the economy are performed with little vigor, and often very imperfectly. Digestion and assimilation are especially obnoxious to this depressing effect, so much so indeed, that however destructive to life heat may be directly, the disease and death produced by it indirectly, through disordered digestion, greatly overbalances all other methods, and swells the death rate during our summers to an alarming figure. I need hardly allude to the fact that the digestive organs of the infant very especially suffer from this cause, as our death records have long taught us. During the heated term of July, August, and September, at least 50 per cent. of our enormous summer mortality, occurs in children under two years of age, and from disorders of the organs of digestion and assimilation. We are taught by observation that there are two methods of controlling this fruitful cause of mortality. We learn that a change of atmospheric temperature of a few degrees lower, has a prompt and remarkable effect in increasing not only the desire for food, but the ability to digest it. This is effected by permitting refreshing sleep, and by diminishing perspiration and its consequent drain upon the nerve forces.

Hence the deduction, that inasmuch as we cannot control atmospheric temperature, we must resort to all available means of keeping down the temperature of the body and of preventing perspiration. This we may accomplish by frequent baths in water of 75° to 85°, removing all obstructing clothing, and by keeping the child as quiet as possible. I know of no one proceeding so invigorating to the heated and depressed infant, as a bath, followed by a nap, made agreeable by a current of air or by constant fanning.

This same idea was urged in this city a quarter of a century ago, as we learn by consulting *Copland's Dictionary*, whose editor says that the child "should be daily exposed to the invigorating influence of the cool, fresh air, and to frequent ablutions with cool water when the weather is excessively hot, and by adapting the clothing to existing temperature. These are important measures, both in the prevention and cure of cholera infantum,

and should never be neglected in the heat of summer, in localities where this disease is endemic." Next to it in utility is the unlimited use of ice-water as a drink. No infant can pass through our heated term, without untold suffering, who is forbidden this luxury, nay, this necessity.

More can be done to husband the vitality of the infant, and to aid its digestion and nutrition, by keeping it cool, than most of us are aware. And yet this most desirable result has been systematically and directly opposed for generations, by the almost universal covering of the body by flannels. We have often stated our belief to be, that infinite harm is done to our babies by keeping their little bodies swathed in flannel, often of the thickest and roughest and dirtiest variety, throughout our summers, and we still entertain and urge the truth of that statement. No infant can be kept as cool and as comfortably at rest in such a temperature, while wrapped in flannel, as when clothed lightly in cotton or linen, nor as cool as the great mass of infants require to be for the preservation of health. Even the specification made in some general rules for public guidance, of "light flannel," does not remedy the popular abuse of the use of this class of coverings during the hot weather of our summers. Thousands of the infants of this city are tortured to death by the heat, the dirt, and the obstructed perspiration resulting from the impermeable character of the flannels habitually used upon them. How this abuse could have been handed down from generation to generation, with the uniform fact standing conspicuously before the world, that the most sudden changes from hot to cool weather are followed by marked improvement in the general health of the people of all ranks, and all dresses, without arresting more general popular and professional attention, we are at a loss to understand. What flannel had to do with the decrease of the death rate of this city from 1,100, for the week ending July, 1868, under a mean temperature of 88°, down to 700 for the very next week, under a rather sudden fall of temperature of 10°, as well as the fall of rain, will be very difficult to say. Will anybody venture to declare that, had the children all been naked during that cool week, the death rate would have fallen less? We trust not. But we will venture to say that, had they been so uncovered, thousands would have been happier and healthier, if not saved from death.

We have long been convinced that, could the rule be enforced of removing every particle of flannel from the skins of our babies, during the months of July and August, and as much more time as the character of the weather would make its absense a source of comfort, vastly more happiness and better health among them would be the result. In no country is Dr. Ballard's fourth aphorism more strictly true than in our own, and especially in this city. It reads as follows:—

"It is an error to suppose (as is popularly held) that sudden changes in temperature are, as a rule, damaging to public health.

A sudden change from cold to *hot* weather is indeed very damaging; but a sudden change from hot to *cold* is one of the most favorable circumstances that can occur, when sickness is regarded broadly as respects a large population."

This remark compares most strikingly and practically with the following untruthful, and misguiding one, from a recent health report, viz.: "Clothing is another important subject for study in relation to health, especially in this climate, where the thermometer sometimes falls, in the *warmest weather*, twenty or thirty degrees in a few hours." * Were it true that we have a climate that is accustomed to changes of temperature from 100° down to 80° or 70° in a few hours, there are no records to prove that it is a source of danger to public health. On the contrary, it always does good, in whatever dress it may find the population, and it is safe to say that those who are nearest naked, are most beneficially influenced by it.

Among our infant population, this law of Dr. Ballard obtains in a most manifest degree. So far, therefore, from there being anything dangerous to apprehend from the changes from hot to cool weather of our summers, they are to be desired as the most salutary occurrences. But as we are unable to command them when desired, it follows as a physiological sequence that we should adopt every available means to secure a similar or an approximative result. Remove the individual from every circumstance which tends to augment or promote the effect of the atmospheric heat, and expose him to the natural agencies for keeping the body cool.† With these suggestions we submit the details of measures to keep our little people cool to the inventive faculties of the physicians, who, after doing all possible to secure that desirable condition, will then have to direct their attention to the food which shall be supplied to the invigorated digestive organs. The importance of adopting all practicable means of preventing the depressing effect of summer heat upon the digestive system of the infant is, perhaps, more strikingly shown by the natural his-

* Third Annual Report, Metropolitan Board of Health.

† The doctrine of securing the health of our infants during the heat of our summers, by preventing the over-heating of their blood and tissues in every possible way, has received a most notable confirmation since this paper was read, in the character of the weather during the summer of 1869, and the mortuary records of this city during the same period. It has been a remarkably cool summer, as compared with that of 1868 and many others, and has had a correspondingly low mortality. If we look for the class which has been thus favored by this cool summer, we find that more than the aggregate gain has been made in children under five years of age, most of it indeed in children under two years. That is to say, that while we have lost a few more adults than during the hot weather of 1868, we have lost so many less infants that the aggregate mortality is far less than during 1868. Were it possible for us to create cool summers, we could control our infant mortality wonderfully; but as that is impossible let us do the only thing left us whose results are next effective, viz.: keep their little bodies cool by frequent bathing and light dressing.

tory of that frightful annual plague of American cities known as cholera infantum than by any other means. This appears to be more an American name than an American disease. Dr. Dunglison says that it is "a disease so termed by American physicians," and that "it differs little, if at all, from what is vulgarly called *Water-Gripes* in England." What we know is, that a very large majority of the cases certified to as cholera infantum in this country present the identical symptoms described in England and in France as characterizing diarrhœa, chronic or acute, of infancy. Since the days of Rush, if not before, we have been familiar with the name, and unfortunately we are very familiar with the disease. So almost invariably is the development of this disease coincident with a high range of atmospheric temperature, that all writers agree that this circumstance is almost if not quite essential. In the history of this disease in our own city, it is a noteworthy fact that, from the earliest mortuary records of the present century down to date, the number of annual deaths ascribed to cholera infantum has always been more or less considerable, with the exception of a single year.*

This exceptional year was 1816, during which but one case of death from this disease is recorded in the Vital Statistics of New York City.

When we seek an explanation of this unusual exemption for that year, the important fact is disclosed "that the summer of 1816 was singularly cold;" that up to about the end of July the weather was so remarkable as to excite general comment in the newspapers of the country, and the temperature ranged from 15° to 20° below that of ordinary years. The highest temperature for the whole summer did not reach 90°, while the mean temperature went but a little above 60°, instead of 75°, the usual mean of New York summer temperature. (Stewart, op. cit.)

When we take this historic fact in connection with the unquestionable one that our infant mortality during summer, from this cause, reaches a far higher per cent. than any of the large European cities, we are led to inquire how our usual summer temperature compares with those cities. We then find that as to high ranges of temperature, it is far more unfrequent to see the thermometer indicate 85° to 88° in London, than it is to see it indicate 100° in New York; and that while the mean temperature of summer in the former city ranges but little if any above 60°, it averages, as we have seen, 75° in the latter. The average London summer temperature, then, is about the same as that which prevailed in our own city during the only year in its history not marked by mortality from cholera infantum.

If we now turn to Paris, it will be seen that for the last two hundred years a record of the rise of the atmospheric temperature of that city to 100° is found but once, viz., 8th of July, 1793. But to come down to more modern times, we find, for example,

* Stewart, Trans. N. Y. Academy Medicine, vol. i., p. 288.

that during 1865, which is recorded as presenting a mean temperature of near 1° above any of the preceding forty-five years, the temperature reached 80° on but two occasions only, the 5th and 6th, and the 14th and 15th of July; and that the mean temperature of the three summer months was about 65°, or ten degrees below the New York average, and about 5° above that of London. Were we to pause here it might, with apparently sufficient reason, be concluded that we had discovered the secret of the fact, that while London and Paris exhibit their greatest mortality during the cold months of the year, New York, as well as many other of the American cities, show their greatest contributions to the grave during the hot months; especially if the additional fact be considered, that this excess of our hot weather mortality is almost exclusively made up from our children under two years of age. We might very justifiably conclude that our appalling summer mortality is exclusively due to our great heat of weather. But we at once encounter facts which at first sight appear to directly oppose this conclusion, if we consult the records of some of the large cities of our own country. New Orleans, for example, which, according to the meteorological tables of the United States Army Reports, has, through a period of twenty years, shown an average summer temperature of about 82°, and a daily elevation to 90° and upwards, does not suffer from cholera infantum to anything like the extent and percentages of the cities of the North, such as Philadelphia and New York. This fact has led authors and practitioners to conclude that other causes than heat help to develop the disease. Says the American editor of *Copland's Dictionary of Medicine*: "In our Southern cities it is even less rife than in Philadelphia, Baltimore, and New York, which proves that it is not dependent on heat alone, as some have supposed." We think there are two reasons why, though heat be the cause of this disease in the Northern cities and country, it does not produce cholera infantum in our Southern cities. The first is, that the style and arrangement of the dwellings in those cities favor the very free circulation of air over and about the population during their long summers. The light airy style of dressing adds greatly to the cooling effect of this free ventilation, so that the population, as a mass, are surrounded by circumstances which enable them to keep their bodies cooler than it is possible for the dwellers in the blocked-up cities of the North to do, though the thermometer marks an actual higher degree of heat. Even the country houses in the two sections, present the same difference of arrangement, so that in the South they are open and airy, while in the North, more perfect ovens than most of the houses in the rural districts are, during the hot days and nights of summer, are hard to find. In all tropical and nearly tropical regions, like New Orleans, these favorable circumstances of habitation and dress, have a marked influence in reducing the effect of the high temperature of the atmosphere, and, as we

believe, really reduce the effective heat below that prevalent during the heated term in our Northern cities, so that the apparent contradiction furnished by the Southern cities to the theory that cholera infantum is more or less directly produced by heat, is not, after all, a contradiction, but a confirmation. Our observations have fully convinced us that we have only to imitate New Orleans in the matter of uncovering the bodies and exposing the skins of our infants to free currents of air during the hot days and nights of our summers, in order to reduce the mortality among them to a rate approximating the infant mortality of that city. The other reason, we think, is found in the fact that the infants of our Southern cities are, as a class, better fed than those of the Northern cities. This results from the well-known fact that among the poor of the South there is found but little of the extreme and squalid poverty and want so often met with in the cities of the North. On this account infants are better cared for by their mothers. Among the wealthier classes the *disposition* and the ability to, and the practice of nursing their offspring, is more generally possessed and encouraged than with us, so that their infants are better fed and cared for in this respect. Added to this is the abundant supply of controllable and faithful wet-nurses from among the colored women in those sections, thus completing the provision for the best and for the most abundant stores of infant food in the cases where the mother cannot or does not nurse her child. We see in this combination of circumstances what to us appears sufficient reason why the population which contributes so enormously to the summer mortality of our great Northern cities furnishes a far smaller *percentum* of the deaths in cities whose range of temperature is higher and much longer continued. It does not in the least invalidate the doctrine that the heating of the blood and tissues of our population, and especially of the infant portion of it, during summer, is the chief cause of summer mortality. To impress us with a proper idea of the potency of this cause of death, as displayed in our own city, we will present it numerically. Taking nine weeks from the first part of the year 1868, during which the weekly death rate ran from 400 to over 600, and another nine weeks from the last part of the year, during which the weekly rate was about 350, we get an average weekly rate of about 400 for these eighteen weeks, which differs little from that of the whole weekly average of the year, excepting nine weeks from July and August, and the first week of September, during which the average per week was about 740, a clear increase of 340 per week, 300 of which, at least, were from children under two years of age, most of them under one year. Having thus, as we think, convincingly shown that, other things being the same, the simple elevation of the temperature to the degree experienced during our summers would produce cholera infantum anywhere, and having indicated the rational means of counteracting its effect to be the uncovering and cooling of the

bodies by all available and physiologically proper means, we wish to pointedly call attention again to the tormenting and destructive effect upon infant existence and life, of the use of flannel next their skins during our intensely hot weather, authority and promulgated rules of Health Boards to the contrary notwithstanding. There is no reason for it found in the history of summer diseases, there is none in well-observed experience, and there is none in the laws of physiology.

But with all we may possibly do to prevent overheating and vital depression, this dangerous condition, in more or less degree, is almost inevitable, manifesting itself in the impairment of all the functions of the economy. Its effects upon appetite, digestion, and nutrition are *most* forcibly brought to the notice of both the people and the physician, though these are mere disturbances of function consequent upon exhausted nerve-power, the real centre of diseased action. As infants, who make up an enormous per cent. of our summer mortality, are especially dependent not only for their maintenance, but for their growth and development, upon the integrity of the functions of digestion and assimilation, the question of food and the ability to digest it is, to them, of the highest importance. Were all women in the enjoyment of the health, the physical vigor, and the functional power which were bestowed upon the primeval specimens of the sex, this question would not be agitating the world at this late date. Unfortunately, however, the influence of advanced civilization, to say nothing of that of the original sin, has, when brought to bear upon women, in all ages, markedly disturbed her ability to nourish her young from her breasts. This being a general truth, we should not be unprepared to witness its illustration in our own land, whose women, as a mass, rank first, probably, among the civilized and enlightened women of the world. Very few practitioners of medicine have failed to discover the decaying powers of our women for the discharge of the functions of a mother, and especially among the highly cultivated and intellectual ones. So marked has this become in our large cities, and in New England, that to-day the lady who can fully nourish her babe at her own breast is the exception, and those who can only in part, or not at all supply its wants, the rule. This ability to nurse is variously estimated, according to the individual experience of various practitioners, from one in two to one in ten of our women. Dr. Nathan Allan, of Lowell, Mass., one of the most industrious and intelligent observers in New England, makes the following observations upon this important subject: "From considerable experience and observation, as well as from inquiries upon this subject, we are satisfied that large numbers of our young married women having children at the present day cannot properly nurse them." He adds the statement that the obstacle is absence or deficiency in the milk, or an imperfection in its quality. "Many women of the present day," says he, "have comparatively no *natural* breasts." "As a

consequence, it is found necessary to feed some infants, in addition to nursing them; others are brought up entirely by hand, and for others wet nurses are procured, and multitudes die because they cannot have the nourishment and care which a healthy and perfect mother can bestow." "This inability of our young women to nurse their offspring presents one of the strongest arguments possible that there is some grand fault in their organization, and that a great change has, in this respect, occurred within forty or fifty years. Aged physicians speak of this as a marked change in their practice. Once our mothers and grandmothers could nurse a large family of children, and raise them too." These remarks are undoubtedly strictly true as respects New England, and indeed of the whole of our country; but from the fact that authors upon the subject of women and children's care and diseases, of one hundred years ago, went more or less extensively into the subject of feeding children by hand, we must admit that some necessity existed for it; so we are forced back to the original conclusion that high civilization—which means high cultivation of the nervous system—is incompatible with the perfect development of the reproductive function. These are points we respectfully suggest for the consideration of the sisterhood,—the "Sorosis,"—leaving them to decide between the encouragement of the development of perfect mothers, and *perfect female politicians*. Whatever may have been the history of the insufficient supply of breast-milk in past times, in other countries, the fact indisputably is, that the necessity for providing for such insufficiency is upon us. As this deficiency has a very direct bearing upon the mortality of our summers, we cannot omit its consideration on this occasion. Admitting the necessity of supplying some substitute for the natural food of the infant, and considering the debilitating effect, at best, of the heat of our summers, we are called upon to direct. Upon physiological and common-sense principles, the milk of some other woman than the mother would be the first choice. This, however, is of but very limited availability, as we all know. A very large number of our women require some aid simply, to fully supply their children. The number is so great that the idea of wet-nursing is quite out of the question; indeed we all know that the demand for wet-nurses is very far in excess of the supply, and we therefore have to look elsewhere for the material for infant food. The milk of some animal is clearly the next choice, and has been from all time among those who have permitted the promptings of nature to guide them. The deductions of chemistry have, however, been much invoked, and the findings of the analyst have been extensively employed as a defence for various kinds and degrees of modification of the milk of the animal, with the view of assimilating it to the healthy milk of the human female. We have been long convinced, and we are by no means alone in this conviction, that this practice has become a perfectly enormous and destructive abuse. It has not only sent thousands upon thousands of infants

to the grave by slow starvation, but much greater numbers have thereby been destroyed by the diseases of the digestive organs induced by crowding into them substances for the digestion of which nature never prepared them. The remarks we shall make, be it understood, refer to the milk of the cow, almost the only kind used in this portion of the world. After due reflection, a very considerable amount of experimentation, and a large experience practically, we pronounce the whole system of diluting milk with water, with the view of rendering it more like human milk, a totally unfounded delusion. It in the first place assumes as fact that which is known to be untrue, viz.: that milk, either of the cow or the human female, is always the same. Nothing is better known than the remarkable variations of both classes of milk under varying circumstances of seasons, food, periods of the day, &c. It in the next place assumes as a fact that which is equally without support in truth, viz.: that the water added is similar in character and organization to the *serum lactis;* and that its addition renders the caseine of the milk, to any appreciable degree, less coagulable and more digestible in the infant stomach. We know that this is not practically true. What is practically true, however, and which one needs not a Newtonian intellect to comprehend, is, that if we add twice as much water to a milk that differs but a small fraction from the human milk, and then allow the child to take the same amount of this mixture that it requires of human milk, we put him upon a starvation diet. And on the other hand, if we take the same amount of cow's milk as the child is wont to consume of breast milk, and add twice the amount of water, with the view simply of giving it in a dilute form, we overwhelm the stomach, tissues, and blood of the infant by a vast excess of fluid, which sooner or later results in serious mischief. The simple physiological and practical fact in all this matter is, that nearly all infants have digestive accommodation quite sufficient to enable them to meet and to easily dispose of the variations continually taking place in the milk produced by the human and by the brute female. It is too well known almost to require mention here, that many females give milk richer in every solid ingredient than some cows. How absurd it would therefore be to dilute such cows' milk. Now, if we come to the consideration of the quality of the milk delivered in our large cities, we at once encounter the fact that, with rare exceptions, it is all diluted before it reaches market, and that at best we have a milk that is below the average quality of cow's milk. The result is, that we are far more liable to starve our infants on city milk than we are to surfeit them. Water can never be added to it with any advantage, but will always result in reducing the food of the infant, to a more or less serious extent. It is simply from the fact that milk thus reduced affords insufficient nourishment, that its dilution with farinaceous decoctions, such as barley, rice, arrowroot, &c., often does better than when water

alone is used. More nutrient material is thus supplied, and if the stomach and other digestive organs of the child possess the required development, it does much better than on simply dilute milk. And these are the only circumstances which afford the slightest evidence that these farinaceous additions to milk improve it as food for the young infant. It is, however, an unphysiological, a totally unfounded, and a very dangerous mistake in practice, to suppose they add essential or useful ingredients to undiluted cow's milk, as food for the infant under six months of age. For more details upon this important subject, of the great and only available substitute for this general failure of natural infant food, we would refer to one of the best papers we have ever seen published, and found in the Transactions of the Medical Society of Pennsylvania for 1868, by Dr. Hiram Corson. The Dr. takes the ground that, both in the cities and in the country, undiluted cow's milk, properly fed, is an excellent and unequalled substitute for the breast, and that with it children may be raised with little if any more difficulty than upon the breast, and we unqualifiedly and heartily coincide with him. We believe that more can be done to reduce our summer mortality by practically admitting this doctrine than by any other one thing. But while we entertain these views, we have a full appreciation of the probability that it will require the united efforts of the profession for a very considerable time to overcome the lactophobia which the swill-milk excitement during the last twenty years has created, and which the teachings and the practice of medical professors have rendered more inveterate.

The public prejudice against milk in general, which the swill reports excited and have kept going, have clearly, in our opinion, done much more harm than good. They have induced the very general adoption of almost any other substance than milkman's milk as infant food, and this has been coincident with a marked increase of summer mortality among those same infants, and from causes which their unnatural diet induces. Even swill milk is undoubtedly better than crackers, farina, arrowroot, barley, &c. It is, in our opinion, clearly the duty of every physician to do his utmost to counteract this result, and to cultivate public confidence in the only available and good infant food provided for us, cow's milk.

We must all believe, and then we shall all teach, that the undiluted milk of the cow is an excellent, and the best generally available substitute for the mother's milk, and so in time we shall convince the people that barley, and arrowroot, and farina, and crackers, and flour, and all that class of articles, are never to be thought of as infant food while milk is to be procured. We shall then prevent untold amounts of infant disease and death during our summers: first, by providing the enfeebled stomach with a proper and easily digested food in sufficient quantity; and second, by diminishing in a notable degree the general and

growing practice of wet-nursing, than which we know of nothing which so directly tends to deprave public morality and to provoke infanticide. If we all believed that our children could be raised as well and as certainly upon the bottle as by wet-nursing, would any of us pay a woman to abandon her own offspring to an almost certain death, and devote her services and substance to the care and development of our children? Few, we presume, will answer this question affirmatively, but many of us, it is feared, too many, alas! answer it at the sacrifice of one human being, as the practice now is. We have tried both the wet-nursing and the bottle-feeding, and as a result of both experience and observation, we would about as soon prescribe poison to an infant, as to encourage its mother to abandon it by offering her money to nurse our child. Could children thus abandoned be intelligently and sufficiently fed by the bottle on pure and good milk, the chances would be excellent for their healths and lives; but we all know that they, as a rule, are exposed to the most abominable neglect, and that consequently they, as a rule, die. We need not dwell upon the various ramifications that this social evil sends forth to the nurture of various degrees of crime. Any reflective mind must see them at a glance. Nor do we possess the happiness of expecting to see the day in which the money of the rich will not induce the poor mother to abandon or otherwise dispose of her offspring, and yield the life and health-giving stores of her breast to the favored infant of the wealthy.

But we do expect to see the practice of wet-nursing abandoned where it always has been, and always must be, a fruitful cause of our summer mortality, viz., in the public institutions. This practice, as is well known, consists of requiring the pauper mother to divide the milk furnished by her own breast between her child and one or more motherless children. The result is, in ninety-nine cases in the hundred, that, unless watched and compelled, in the language of the warden of one of these institutions, to give "their due share of attention to the orphan children," such orphan children are left to die of starvation, or to be destroyed by improper feeding. But if watched, and made to duly distribute her care and substance, the process of slow starvation is commenced in two or more children at once, and the death of all may be almost certainly predicted. This might all be expected by any one who will reflect upon the fact we have already mentioned, that large numbers of our women have barely sufficient milk for one child; and upon the additional fact that women are very rare indeed in this country who can properly nurse two children. The rule should therefore be, that no woman in our public institutions should ever be required to breast-feed more than one child. If she have a child, that of course should have the breast, thus giving it a fair chance for life. She, however, is able generally to take very good care and properly bottle-feed one or two infants besides her own. If for so doing she be allowed a reasonable remunera-

tion, in the form of a salary or premium, we fully believe most satisfactory results would be obtained. But overtaxing the human breast, in the attempt to supply our pauper and orphan infants with wet nurses, can, in the very nature of things, result in nothing but disaster, in this city and country. We well remember the flattering promises held out to us some time ago, from the effort to wet-nurse one of our public institutions for infants, but never believing in the practicability of the plan, we were not surprised to read that the result of the effort, in the language of the reporter, was a "mortality scandalous to the profession and to the department." Let mothers nurse their own children while they have milk, taking it for granted that they never have too much. Let the orphan children have all the good undiluted milk of the cow they will take, and cold water when they need it, and we have done all we can do for them in the way of food. We forbear entering into further detail, for a word to the wise is sufficient. Keep our adult and our infant population as cool as possible during the intense heat of our summer. Encourage every mother to nurse and take care of her own children, if possible, and discourage wet-nursing, in both private and public, by every possible means.

When the infant is only partly or not at all supplied by the breast, give it all the best cow's milk it will take, and our summer mortality will be very greatly diminished.

STATED MEETING, JULY 1, 1869. DR. H. D. BULKLEY, PRESIDENT, IN THE CHAIR.

DRS. GEO. M. BEARD, ALBERT H. GALLATIN, E. G. LORING, CHARLES WRIGHT, CHARLES J. PARDEE, and JEREMIAH C. MORTON were elected Resident Fellows.

DR. POST exhibited some specimens of an Oriental remedy for epilepsy, a bulbous root, the common name of which is the Leaven of March, the botanical name Bonyardia Chrysogonon, of the natural order Berberidaceæ. The natives of Beirût, Syria, use it in drachm doses, in infusion, every three or four hours, until it causes vomiting.

The discussion of the paper on Summer Mortality, read by Dr. Rogers at the previous meeting, was opened by the President, who was followed by Drs. Blumenthal, Rogers, Peaslee, Garrish, and Neftel.

The Academy then adjourned.

STATED MEETING, SEPT. 16, 1869. DR. H. D. BULKLEY, PRESIDENT, IN THE CHAIR.

DR. SAMUEL H. ORTON was elected a Resident Fellow.

DR. HANBURY SMITH read a paper on

MINERAL WATERS AS THERAPEUTIC AGENTS.

In answer to a question regarding the properties of the Missisquoi Springs, Dr. Smith said that he had never been able to discover any properties in this water other than its remarkable purity, being entirely free from saline or earthy matter; and as far as he had been able to learn, the same had been the experience of other chemists.

DR. FARNHAM spoke of the Caledonia Springs, remarking that by the use of the water, both internally and in the form of baths, he had been relieved of a distressing eczema.

DR. GARRISH called attention to the Massena Springs of St. Lawrence County.

DR. SMITH said, in reply to a question by Dr. Garrish, that two pints of mineral water, in divided doses, was the proper quantity to be taken during twenty-four hours. Persons often drink too much and too rapidly, whereas the quantity should be as above stated, and in order to receive the greatest amount of benefit, the patient should drink slowly, and take some exercise after each draught.

The Academy then adjourned.

STATED MEETING, OCTOBER 7, 1869. DR. H. D. BULKLEY, PRESIDENT, IN THE CHAIR.

DRS. NATHAN BOZEMAN and GEORGE THOMPSON were elected Resident Fellows.

DRS. WM. B. LEWIS, MARCUS P. STEPHENSON, CHARLES A. HART, CHARLES J. PARDEE, JEREMIAH C. MORTON, and JUSTUS J. SPRENG were inaugurated.

DR. J. R. LEAMING read the following paper on

PLEURITIS.

Those in the habit of making autopsies are aware of the frequently recurring evidences of former pleuritis, and, too, in subjects where it had not been suspected during life, although the patient had been under competent medical observation. The practical auscultator finds in his examinations that he passes his ear over but few diseased chests without noting evidences of pleuritic adhesions or thickening, and frequently he can trace the history of the pleuritic lesion back to a period anterior to the

disease of the lung. I am not referring now to those intercurrent attacks of pleuritis which are merely side issues in the course of organic changes in phthisis, but of those attacks of the subacute disease which give so little warning of their presence by pain or constitutional disturbance, but which, by crippling the action of the lung in binding it to the diaphragm and costal parietes, determine and hasten the final result of tubercular consumption. These facts directed my choice of subject for this paper, and yet I shall not treat it systematically, nor particularly as to the minute anatomy, interesting as this field is, where *all* its hidden treasures are as yet unexplored, and mines of great wealth still await the earnest seeker after truth; but pass to some points of practical interest, and to others of singular phenomenal occurrence.

Pleuritis may be studied in four divisions—the acute, the subacute, the circumscribed, and the intercurrent. The acute and subacute are general, the circumscribed is partial, and the intercurrent may be either partial or general.

Acute pleuritis is the only variety popularly recognized; it is violent and fierce in its whole course, frequently ushered in with a chill, which is immediately followed by burning heat, aching pains in the head and limbs, but more particularly by the characteristic stitch in the side. From the beginning it is accompanied with congestion of the lung, and, unless aborted by heroic treatment becomes pleuro-pneumonia. Indeed, it is owing to the pulmonic complication that the symptoms are so violent. The rigors, the pain, the elevation of temperature, and the stitch are the recognized rational signs, and their succession is frequently so rapid that they seem to come all at once.

The auscultator, should he see the patient early, will discover signs indicating coming trouble prior to the appearance of the rational signs. The true respiratory murmur will be altered, muffled, and diminished, just as soon as the hurrying congestion commences. And before the stitch or the great elevation of temperature, the dryness of the pleura will cause a little *jar* as the sound vibrations from the bronchiæ pass through the pleura and chest-walls to the ear. But, the disease once established, lymph thrown out upon the pleural surface, and exudative matter into the true respiratory system of the lungs, with effusion of serum into the cavity of the pleura, the activity of the rational signs subsides, the pain disappears, and the temperature falls back again to its natural standard, but some of the physical signs remain. In three or four weeks the exudative matter will have been resorbed or expectorated, the serum in the pleural cavity will have disappeared, and the lymph alone remain, to cause thickening of the pleura, or adhesions to be slowly removed by time.

I would not have taken up so much of your time in describing this common form of pleuritis, were it not necessary in order to show the importance of the treatment proposed. This form of disease occurs in vigorous subjects, and is frequent in the hill countries,

where the air is pure and bracing, and the circulation is vigorous. After some unusual exposure to cold and wet, when the system is unprepared to resist and throw off these influences, on account of some temporary fault of the digestive organs, or great fatigue with mental depression, perhaps, the disease makes its attack. The prodromata have been unnoticed, if not unfelt, and the onset is sudden. Now is the time for vigorous treatment, to arrest this inflammation before lymph is thrown out to damage the lung by adhesions. General bleeding, cupping or leeching, to be followed by a large dose of calomel, and this dose must be large. It is not given for the purgative effect; indeed its best effects are obtained where it does not produce copious discharges from the bowels. But it is needed to produce a profound impression upon the great organic nervous system, which is the mysterious life of the body, and which governs and guides all secretions as well as growth and decay, but is now, as it were, insane, and about to destroy what it should preserve. Nothing brings it back to its right mind like the sedative dose of calomel. You do not wish the constitutional effects of the medicine—that would be disastrous—but you *do want its powerfully sedative conservative* effect, and the dose must be proportioned to that end. It must be large—twenty, thirty, or forty grains may be given with entire safety, according to the constitution and idiosyncrasies of the patient and the violence of the disease. You strike one vigorous blow, you exorcise the demon, and the work is done. If the stitch be not relieved, or there remain some pulmonary congestion, a large blister of cantharides will give perfect relief, and after a few days of rest the patient will be restored to perfect health; the lung not crippled, nor damaged by pleuritic adhesions.

To appreciate the value of the abortive treatment, suppose the time for action has passed, and nature has struggled alone and without assistance over the inflammatory stage, and effusion of lymph, serum, and exudative matter has taken place. The damage is done, the lung is crippled, the battle has been fought, and the demon of disease has conquered, so that the poor patient, if allowed to live at all, must do so in a reduced state of health, with vitality permanently lowered. Active treatment now would be out of place, and worse than useless—it would be injurious. There is nothing left for us now but conservatism. Calomel, blisters, and diuretics only impair still further the already damaged organs of secretion, as well as those of supply. Our whole duty is to repair damages—that is, we allow nature to do it, and remove obstructions from her way, by assisting the digestive organs, and in so doing, equalizing the circulation. It is bad treatment now to persist in the use of depressing agents, but especially wrong is the continued use of blisters. When the inflammatory tendency is overcome by exudation and effusion, it is certainly unwisdom to re-excite the still irritable pleural surface, and, perhaps, cause further exudation of lymph, to bind down the

already disabled lung with double bands, as I really think is done in such cases.

It would be better to forego the advantages of blisters in the abortive treatment, if by so doing their improper use could be prevented afterwards.

Subacute pleuritis is seen mostly in city practice; it is in the hospitals, frequents the dispensaries, and occurs in private practice. Its subjects are walking patients—complain perhaps of being "bilious," and get out of breath on slight exertion. They have poor appetites, their tongues are furred, their digestive organs are all wrong, and they seek advice of the practitioner, or at the dispensary, where it is found, upon examination, that the pleural cavity is filled, or partly filled, with serum, and if it be on the left side, which is generally the case, there is dislocation of the heart and consequent interference with the circulation. Close cross-questioning may bring out the fact that the patient—generally a laboring man—has not been well for some time, perhaps for weeks, but has kept at work till within a day or two, and even now he can hardly say *where* or *how* he feels ill, but he knows that he has grown worse. Occasionally the effusion takes place whilst the patient is under medical observation, but it comes so quietly, and makes so little disturbance, that its presence, when discovered, surprises the physician even more than the patient, so that whether the patient apply for the first time, with the effusion already taken place, or whether it appear after he has entered the hospital, or whilst he is a regular attendant at the dispensary, or at a physician's office, still the history of the beginning of the disease, the cause why, will be somewhat involved in doubt. That it is the result of temporary obstruction to the circulation, which has been relieved by the effusion, is, no doubt, true; but whether there was congestion of the liver, or kidneys, or spleen, or merely gastric derangement, it will be difficult to determine. Cases of subacute pleuritis, with adhesions, go through their whole course, and get well, without being recognized, either because the practitioner's attention was not directed to this condition, or that the patient had never felt ill enough to consult a physician. The frequency with which we find pleuritic adhesions in autopsies, not suspected during life, makes it certain that this serious result—serious in its consequences, by lowering the general vitality—may never express itself in rational signs, physical examination *alone* can make it known with certainty.

Subacute pleuritis, by lowering the vital standard, has much to do in influencing and determining the future tubercular condition of the lungs, and makes the consideration of this part of our subject of grave importance. In our experience, pleuritic adhesions have been a determining cause in a majority of the cases of consumption that have come under our observation. This does not invalidate the doctrine of inherited or acquired proclivities, for it is then that this quiet effusion is liable to take place, and it is in

those cases alone that it becomes a determining cause. It might be supposed, *à priori*, that a cause which prevents the properly filling of the lung with air, and interferes with the oxygenation of the blood, must tend to its depravity, and consequently to the formation of tubercle. At all events, whichever may be the favorite *rationale*, the practical fact, I imagine, will not be disputed, that a damaged lung from adhesions is a frequent source of tubercular consumption. It is a determining condition that correct treatment and proper management may remove, and consequently these cases of tubercular phthisis are preventable, a fact of the highest importance.

How shall we prevent the occurrence of pleuritic adhesions, or, after they have occurred, how shall we get rid of them, or prevent their most damaging effects? In the first place, we must not forget that nature, in subacute pleuritis, takes pains to prevent adhesions, by keeping the pleural surfaces apart by the effusion of serum; that this effusion remains till the lymph, which is exuded at the same time, has become so changed that adhesions do not occur, or, if they occur, they are not so strong as they would have been had the pleural surfaces been brought together earlier. It is unwise to remove the serum, either by medicines or by the canula, while the danger of adhesions continues. This is a point not understood, or much neglected in practice, for the opposite course is taken, and the object of the practitioner seems to be to get rid of the fluid at the earliest possible moment, and to this end he directs his battery of diuretics and blisters. The result is, he damages the patient more, tenfold, than the disease. For nature in almost all cases, if not interfered with, will, in due time, remove the fluid with the least possible harm. One great means of preventing phthisis will be to stop blistering in subacute pleuritis after the effusion of lymph, and study more intelligently the action of nature. Another evil of this officious intermeddling with the action of nature is, the effect it may have upon other organs, and especially upon the kidneys. The effect of blisters of cantharides and diuretics, especially when the kidneys are in a morbid state of irritability, must be to damage the secreting apparatus and delay the resorption of the fluid, or prevent it altogether, and allow it to become purulent. Usually the fluid is removed by the unassisted action of nature; but if she fail, we are not called upon to again stimulate her over-taxed and debilitated organs, trying to force her to do what she has already failed to accomplish from inability, but should remove the fluid by artificial means.

Some years ago, before I had learned these truths, I had a patient of strong constitution, who was laboring under great mental depression when I was called to see him. Two or three days previously he had taken a long drive into the country in an open carriage, and he thought his present illness the result. He had no cough, his respiration was in nowise hurried, neither was

his pulse accelerated, but his skin and eyes were tinged yellow, and his tongue was furred. I did not put my ear to his chest, so little did I suspect any trouble there. I did not see him again for two days, and then his respiration was hurried, but the skin was cool and the pulse not increased in frequency. Physical examination discovered that the right pleural cavity was partly filled with fluid, and believing it my duty to try to remove the fluid as soon as possible, I *pushed* the treatment to that end. I used mercurials and diuretics, and especially blisters of cantharides. I changed them from place to place till the whole side had been covered, yet the fluid obstinately refused to be removed. About two weeks after I first saw him, he became intensely excited, and I was summoned hurriedly to his bedside, and found him gasping for breath and almost pulseless. Physical examination showed that the effusion then completely filled the cavity of the right pleura; he had had no pain from the beginning; his distress was from dyspnœa only. I then called the late Dr. Cammann in consultation, and he advised me to cease all active treatment, and merely pay attention to the digestive organs. He slowly recovered. About one year from this time he was shot down in the street, and was taken to Bellevue Hospital, where he died. A post-mortem examination was made by Dr. Eugene Peugnet, the house-surgeon, who told me that he found the right lung firmly bound down by strong adhesions. I was forcibly impressed at the time, and all my observations since have tended to deepen those impressions into firm convictions, that my ill-judged and persistent zeal in trying to remove the fluid a year before, was the cause of the permanent disablement of that right lung. I believe that had I let him alone, merely attending to his general health, and waited for the natural resorption of the fluid, the lung would not have been bound down, but would have expanded and filled the pleural cavity again, as in health. The blisters, I am convinced, caused renewed exudation of lymph, which fastened the lung down in its compressed condition, and left it forever damaged. There was no phthisical tendency in his constitution, and his violent death rendered it impossible to study the history of the case as to any acquired tendency; but one thing is certain, he could never have regained his former vigorous health.

How shall we treat a case when adhesions have taken place, in order to regain, as nearly as possible, the normal state of health, and prevent the tendency to phthisis? The digestive organs must be kept as far as possible in healthful action. The use of the compound iodide of potash ointment, or turpentine externally, in the neighborhood of the adhesions, and the long-continued use of muriate of ammonia, which improves the digestive powers and has no depressing action, is very well. But of much greater importance is the habit of forcibly filling the lungs with air, which is done in this wise: raising the arms above the head, taking a full inspiration, and holding the breath, bring them downwards and out-

wards till they press upon the side, before allowing the breath to escape. This fills the whole lung to its utmost capacity, which is difficult to do in any other way. When we take an inspiration with the hands above the head, and well back, the most of the lung will be filled to distention, but the summit of the lung may yet remain partially collapsed; but bringing the arms downwards and outwards, while holding the breath, and pressing them upon the sides, the summit of the lung will also be distended, and this accomplishes about all that can be desired in physical exercise. A trial of this method will satisfy any one that it largely increases vital capacity, and, of course, diminishes tubercular tendencies. Another end is promoted by so doing, of no small consequence: frequently filling the lung will elongate the adhesions, and, in so far, relieve the crippled condition of the lung.

What are the signs of pleuritic adhesions or pleuritic thickening, where the chest has retained or regained its normal dimensions and contour? These must be sought for in the respiratory murmurs and in the percussion note. If there be adhesions, and the lung have motion, the stretching of the adhesions produces a peculiar sound like subcrepitus, but with this difference,—all the crackles, which are generally soft and like tearing flannel, *will be upon one plane*, and will *move in one direction*. When the adhesions are firm, and there is but little motion of the lung, there may be subcrepitant râles, but the broncho-respiratory murmur will come to the ear, in inspiration, altered, harsh, and jarring, and the same sign will occur in thickening, and the percussion note will be sonorous, flat, and wooden.

Intercurrent pleuritis is that which takes place in the course of grave structural disease of some other organ, as in disease of the heart, or the kidneys, or in tubercular phthisis. When it occurs in disease of the heart or kidneys, its tendency to plastic deposit is not great, and its damage to the lungs by adhesions of little consequence. The effusion of serum is temporary relief to the diseased organ. But the intercurrent pleuritis of tubercular phthisis requires more consideration. It is always partial and generally of quite limited extent. It is characterized by exudation of lymph, forming strong adhesions, and these must be regarded as conservative. In the first place, they arrest the progress of tubercularization, acting as a derivative, and enforcing rest locally; and secondly, by preventing perforations of the pleura, and the deplorable accident of hydro-pneumothorax. We would not attempt to break up these adhesions if we could; on the contrary, we would like them to be as firm as possible. The treatment should be limited to the relief of pain.

Circumscribed pleuritis is of course always partial, and is mostly complicated with lobular pneumonia. It is readily diagnosticated by the limited dull space and respiratory sounds, which is surrounded at its edge by the ring of characteristic subcrepitant râles, which denote stretching of pleuritic adhesions. It may be

mistaken for tubercular deposits, but a little attention will prevent this error. It requires no special treatment, always getting well in due time. But it sometimes presents some acoustic phenomena of interest.

Dr. Scott Allison, a few years ago, introduced to the profession a small acoustical instrument called a hydrophone. It is an elastic bag filled with water, and it is serviceable on the principle that compressed fluid receives and conveys sound vibrations with truth and fidelity. He used it by placing it between the object-end of a Cammann's double stethoscope and the chest-wall, and found that it conveyed respiratory and heart sounds with distinctness to the ear, even through thick clothing. This principle of compressed fluid conveying sound in a marvellous manner, is sometimes exemplified in circumscribed pleuritis. There was such a case in one of the surgical wards of St. Luke's Hospital about two years ago. A man had circumscribed pleuro-pneumonia on the right side, posteriorly and inferiorly, with the singular phenomena of tympanitic resonance over the space of circumscribed pleuritic effusion. The ear recognized the dull space, and the absence of respiratory murmurs, and the ring of subcrepitant râles indicating pleuritic adhesions surrounding it; but percussion, instead of the dull flat sound usually heard over effusions, brought out a clear, loud, tympanitic resonance, to the astonishment of the auscultator; but a little further investigation showed that the colon, which was directly in front, was filled with gas. The effusion filled the sack to its utmost tension, forming a hydrophone, and the compressed fluid conveyed the impulse through the consolidated lung to the colon, and there formed the tympanitic resonance, the sound vibration which was returned through the same excellent medium, and produced the delusion of the tympanitis, directly under the finger. As soon as resorption of the fluid commenced, it relieved the tension of the circumscribed sac; the tympanitic resonance grew less, and finally disappeared.

Another case, in the practice of my friend Dr. W. W. Jones, was equally surprising and instructive. A man standing in his stable, and giving orders, had the stalk of a timothy head between his teeth, and suddenly inspiring while talking, the timothy head passed through the larynx into the lung. It caused violent coughing, which continued all that night. Dr. Jones saw him in the morning, but the physical signs indicated merely bronchitis, which was in both lungs. These symptoms continued three or four days, when there were added signs of pleuritis at the lower and back part of the right lung, which were followed by those of consolidation of the lung. After about a week, suddenly, at night, about two quarts of fluid of a gangrenous and fœcal odor were discharged from his mouth, and in this matter was the denuded stalk of the timothy head. The discharge continued to be profuse, and the odor offensive, but the physical signs were changed. At the upper part of each lung, and throughout the left, the signs con-

tinued to indicate bronchitis, but in the right lung these signs rapidly changed, in passing the ear downwards, to those of consolidation at the middle of the lung, where the respiration was of a tubular character, and below amphoric and blowing, with very liquid gurgling. Dr. Jones describes the sound as very peculiar—the blowing gurgling sound passed downwards, apparently to the bottom of the lung, where it seemed to stop for a moment, and then went on downwards. I saw the case, with Dr. Jones, on Saturday the 25th of July, 1868, and the following is copied from notes made at the time: Respiration harsh in both lungs, and prolonged in expiration, with a rhonchus of low pitch, flat in character and short in duration. Posteriorly on the right side, near the inner angle of the scapula, but a little without, there was harsh bronchial breathing, which rapidly increased in intensity downwards, becoming tubular and then amphoric, with loud and very liquid gurgling, with amphoric resonance and reverberation, to near the tenth rib, where it seemed to have its maximum intensity. Still the amphoric respiration and very liquid amphoric gurgling continued, with great distinctness, to be heard so far down as below the false ribs. Near the tenth rib, at the inspiration, there was a valvular sound or flap, and then the amphoric blowing sound and gurgling seemed to go downwards, as though the air passed through an opening into a larger space. The percussion note was of moderate resonance from above downwards to near the lower part of the lung, where there was some dulness, but there was resonance below the diaphragm. The amphoric blowing sound could be heard in front, but was distant from the ear.

The theory I formed was, that the stalk of the timothy head had passed through the larynx and trachea into a bronchus, and had been thrust farther and farther by the spasmodic coughing, till it had pierced the pleura and diaphragm, and passed into the liver, and as a result an abscess had formed in the liver, which discharging brought up with it, through the lung, the timothy stalk and seeds, as were found in the contents of the abscess. The post-mortem examination showed that the stalk had pierced the pleura, but had not pierced through the diaphragm—that diaphragmatic pleuritis had taken place and was circumscribed by adhesions. This formed a chamber below the lung and above the diaphragm, which was filled to tension with fluid and air. The amount of fluid was limited by the size of the chamber, but each respiration forced some fluid and air into this artificial chamber, while at the same time some of the fluid contents were forced up and expectorated; and the motion of respiration caused the valve-like sound which was heard with the gurgling. The colon and intestines were found filled with gas, and this completed the acoustic conditions, and accounted for this remarkable consonation of sound. The amphoric gurgling and blowing resonance, taking place in the cavity of the lung, was conveyed by the com-

pressed fluid in the artificial chamber to the intestines, distended with gas, was loudly consonated, and returned by the same medium to the ear below the diaphragm. The deception was complete.

This hasty review of the subject leads us to the following conclusions: That the acute form of general pleuritis is always complicated with pneumonitis, and is, properly, pleuro-pneumonia. That its onset is sudden and violent, and unless it be aborted, it cripples the lung, binding it down with strong adhesions. To abort the disease, the treatment must be heroic—bleeding, general or local, large doses of calomel, tartar emetic, aconite, veratrum viride, or some other means must be used to arrest inflammatory action, and before exudation of lymph has taken place, or not at all. The medical attendant who hesitates from want of knowledge, or courage to make use of these powerful agents at the proper time, to save the patient from damage, may be a good nurse, but he can hardly lay claim to the noble title of physician. But should the lung, unfortunately, have suffered from adhesions, the time for heroic treatment has passed, and attention now must be paid to the health of the general system, and especially to the digestive organs.

Subacute pleuritis or uncomplicated pleuritis is generally unaccompanied with pain, or much constitutional disturbance. That its insidious advance is all the more dangerous that it gives little warning of its attack. That if not treated at all, or inefficiently, it will leave the lung damaged by adhesions; and that if treated wrongfully, by blisters of cantharides, the strength of the adhesion may be increased, and the lung be more hopelessly crippled. That the effusion should not too soon be withdrawn, as one or other of two conditions will then pretty certainly ensue; either the pleural surfaces will be glued together by strong adhesions of lymph, or there will be a renewed effusion of serum. That it is an important fact that pleuritic adhesions are a frequent cause of tubercular phthisis; that proper treatment may relieve the lung from these adhesions, and, consequently, that this form of phthisis *is preventable.*

That intercurrent pleuritis occurring in tubercular phthisis is only a conservative act of nature.

And, finally, that some peculiar and apparently unaccountable physical signs attendant upon circumscribed pleuritis, are, really, but legitimate manifestations of acoustic law.

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*Barker, Luke, Dec. 13, 1849.
Barry, Robt. A., Physician to Flushing Insane Asylum.
Barstow, J. W.
Bayles, George.
Belden, E. B.
*Batchelder, J. P., April 7, 1868.
Baylies, Bradford L. B.
Beadle, Edward L., Physician to N. Y. Institution for the Blind.
Beales, J. C.
*Beales, Gorham, January, 1848.
*Beck, John B., April 9, 1851.
Bedford, Gunning S.
Bell, A. N., Physician to Brooklyn City Hospital.
Bibbins, W. B., Physician to Demilt Dispensary.
Blakeman, Wm. N.
*Bliss, James C., Aug. 12, 1855.
Bliven, J. P., Surgeon, Metropolitan Police.
Blois, Samuel.
Blumenthal, Mark.
*Bolton, Jackson, Feb. 16, 1866.
Booraem, Augustus C.
*Boyd, Thomas, March 18, 1856.
Bozeman, Nathan.
*Brady, Patr. J., Oct. 24, 1856.
*Brooks, Geo. W.
Brown, D. T., Physician to Bloomingdale Insane Asylum.
Brown, James L.
Brown, Wm. K.
Brueninghausen, Chas.
Buck, Gurdon, Surgeon to N. Y. Hospital.
Budd, Chas. A., Prof. Obst., Med. Dep. N. Y. University.
*Bullus, Edw. S., Sept. 25, 1854.
Bulkley, H. D., Physician to N. Y. Hospital.
Bumstead, F. J., Ophthalmic Surgeon, N. Y. Eye and Ear Infirmary.
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Burrall, Fred. A., Physician to Charity Hospital.
Burtsell, Thomas E.
Byrne, John.
*Cameron, James, Dec. 12, 1851.
*Cammann, G. P., Feb. 14, 1863.
*Campbell, James, Mar. 12, 1853.
*Carrington, Wm. A., July 17, 1866.
Carter, Galen.
Chalmers, Thos. C.
Chamberlain, W. M., Physician to Demilt Dispensary.
*Chapin, John R., June 23, 1852.
*Cheesman, John C., Oct. 11, 1862.
*Childs, Timothy, September 3, 1865.
Church, Allen S.

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*Churchill, Chas. W., Oct. 27, 1859.
Clark, Alonzo, Prof. Theory and Practice, Coll. Phys. and Sur.
Clarkson, C. V.
Clements, Jas. W. G., Physician to N. Y. Institution for the Blind.
Cochran, George, Surgeon, Metropolitan Police.
*Conant, D. S., Oct. 8, 1865.
Conolly, J. J.
*Cooper, James S., Physician to Association for Relief of Respectable and Indigent Females, April 11, 1867.
*Covel, John C., Nov. 4, 1860.
*Cox, Henry G., Consulting Physician, Emigrants' Hospital, May 29, 1866.
Crampton, Henry E.
Crane, James, Physician to Brooklyn City Hospital.
Crane, John J., Surgeon to Bellevue Hospital.
*Creveling, A., April 28, 1852.
Dalton, Edward B.
Dalton, J. C., Prof. Physiology, Coll. Phys. and Surg.
Dana, Samuel W.
Davis, John.
Delafield, Edward, Pres. and Emeritus Prof. Obst., Coll. Phys. and Surg.
Derby, E. W., Physician to Eastern Dispensary.
Detmold, William, Prof. Military Surgery and Hygiene, Coll. Phys. and Surg.
*Donaghe, W. R., July 18, 1866.
Douglas, J. Hancock.
*Douglas, Robert, July 25, 1861.
Downs, Henry S.
Drake, Benjamin.
Draper, John C., Prof. Chem., Med. Dep. N. Y. University.
Draper, J. W., Pres. and Prof. Phys., Med. Dep. N. Y. University.
Draper, Wm. H., Phys., N. Y. Hospital.
Dudley, Wm. Henry.
Dunster, Edward S.
*Dwight, Wm. W., July 11, 1861.
*Earle, Edward, Aug. 21, 1849.
Elder, Alex.
Eliot, Elsworth.
Elliot, Geo. T., Prof. Obst., etc., Bellevue Hosp. Med. Coll.
Ellis, Sam. C.
Elsberg, Louis.
Emmet, Thomas Addis, Surgeon, N. Y. Woman's Hospital.
*Enos, DeWitt C., Prof. Anat., L. I. College Hospital, Dec. 4, 1868.
Farnham, H. P.
*Ferguson, John T., Oct. 11, 1859.
*Ferris, T. Floyd, Nov. 6, 1855.
Field, H. M., Physician to Demilt Dispensary.
Finnell, T. C.
*Fisk, Lyman, June, 1859.
Fitch, James D., Physician to Colored Home.
Flint, Austin, Prof., Bellevue Hosp. Med. Coll.
Flint, Austin, Jr., Prof. Phys., Bellevue Hosp. Med. Coll.
Foster, Joel.
Foster, S. Conant.
*Foy, Michael E., June 9, 1861.
*Francis, John W., Feb. 8, 1861.
Francis, Saml. Ward.
Freeman, N. M.
Furman, Guido.
Gallatin, Albert H.
Gardner, Aug. K.
Garrish, John P.
Gescheidt, Anthony.

* Deceased.

Gilfillan, William, Prof. Therap. and Mat. Med., L. I. College Hospital.
*Gilford, Jacob T., June, 1869.
Gomez, Horatio, Phys., N. Y. Dispensary.
Gouley, J. W., Surg., Bellevue and St. Vincent's Hospital.
*Graham, John, May 20, 1847.
*Green, David, October 18, 1856.
*Green, Horace, Nov. 29, 1866.
Green, John W., Physician to Bellevue Hospital.
*Greene, Isaac, July 3, 1854.
Griscom, John H.
Guernsey, Peter B.
Gunn, A. N.
Hackley, Charles E.
Hall, Edwards.
Hall, Samuel.
*Halsted, J., April 10, 1856.
Hamilton, F. H., Prof. Surg., Bellevue Hosp. Med. Coll.
Harris, Elisha, Sanitary Superintendent Met. Bd. of Health.
*Harsen, Jacob, Dec. 31, 1862.
Hart, Charles A.
*Hart, John, Aug. 9, 1867.
*Hazlett, John.
Hedges, David B.
Henschel, Charles.
Herrick, Everett.
Herzog, Max, Physician to German Dispensary.
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Heywood, Chas. F.
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*Hobart, Wm. H., Jan. 21, 1857.
*Hogan, Dan'l M., —— 1849.
Hogan, Michael.
Holcomb, Wm. F., Ophthalmic Surgeon, Demilt Dispensary.
Horsfield, Thomas W.
Howard, Benjamin.
*Hunter, Abm. T., August 1, 1849.
Hubbard, Samuel T.
Hull, J. G.
Humphreys, E. K.
Husted, Jarvis N.
Husted, Nathl. C.
*Hutchinson, E. F., March 2, 1848.
Hutchison, J. C., Prof. Surg., etc., L. I. College Hospital.
Hyslop, James.
*Isaacs, Chas. E., June 16, 1860.
Ives, George W.
Ives, John.
Jackson, Wm. H.
Jacobi, A., Clinical Prof. Diseases of Children, Med. Dep. N. Y. University.
Janes, Edward H.
Janvrin, Joseph E.
Jenkins, J. Foster.
*Johnson, W. J., September 22, 1860.
*Johnston, F. U., Jan. 7, 1858.
Jones, E. Lee.
Jones, Wm. W., Physician to Nursery and Child's Hospital.
Jones, A. S., Surgeon, Metropolitan Police.
Kammerer, Joseph, Physician to German Dispensary.
*Kearny, Ravaud, —— 1850.
Keene, Stephen S.
Kennedy, James, Surgeon, Metropolitan Police.
Kerrigan, Joseph A., Physician to St. Vincent's Hospital.
Kilbourne, J. Sage.
Kimbark, Everett H., Surgeon, Metropolitan Police.
*Kingsbury, G. H., May 4, 1852.
*Kissam, R. S., Nov. 28, 1861.
Krackowizer, E., Surgeon, Jews' Hospital.

* Deceased.

Lawrence, J. S.
Leale, Charles A.
Lee, Charles C.
Leaming, James R., Physician to Demilt Dispensary.
*Leo Wolfe, George, March 14, 1855.
Leo Wolfe, Morris.
*Leveridge, Benj. C., April 16, 1862.
Lewis, William B.
Lidell, John A.
Linsly, Jared, Consulting Physician to Asylum for Lying-in Women.
Little, Jas. L.
Livingston, W. C.
Loines, J. P., Physician to Eastern Dispensary.
Loomis, Alfred L., Prof. Theory and Practice, Med. Dep. N. Y. University.
Loring, Edward G., Jr.
*Lyon, Jas. L., Dec. 24, 1858.
MacGregor, J. R.
*Manley, Jas. R., Nov. 21, 1851.
Markoe, Thos. M., Surgeon, N. Y. Hospital, Adjunct Prof., etc.
*Martin, Joseph, Apr. 26, 1864.
*Marvin, Dan'l D., Oct. 21, 1852.
Maxwell, W. H.
*McAllister, G.
McClellan, Christopher R.
McClelland, John.
McCready, B. W., Prof. Mat. Med, &c., Bellevue Hospital Med. Coll.
*McDonald, Jas., May 5, 1849.
McDonnell, Patrick W.
McLeod, S. B. Wylie, Physician to Asylum for Lying-in Women.
McMillan, Chas., Physician to Orphans' Home, etc., Prot. Epis. Ch.
*McNeven, W. H., May 12, 1854.
McNulty, John.
*Meikleham, D. S., Nov. 20, 1849.
Merritt, J. K.
Metcalfe, John T., Consulting Physician to Bellevue Hospital.
Miller, Francis.
*Miller, John, Jan. 13, 1863.
*Miller, W. Ellison, Jan. 16, 1852.
*Miner, W. W., March 20, 1863.
*Miner, William, Nov. 16, 1859.
Mitchell, C. L.
Monell, J. A.
*Moore, Sam'l W., Aug. 26, 1854.
*Moran, Thomas, —— 1853.
Morris, R. L.
Morton, Jeremiah C.
*Mott, Valentine, Apl. 26, 1865.
Nelson, James B.
*Neilson, John, June 19, 1857.
Nichols, Truman.
Nichols, Elias S.
Noeggerath, E., Physician to St. Franciscus Hospital.
Noyes, H. D., Surgeon, N. Y. Eye and Ear Infirmary.
*Ogden, Benjamin, June 18, 1867.
Olmstead, Roger S.
Ordronaux, John.
Osborne, Charles H.
Otis, F. N., Surgeon, Metropolitan Police.
*O'Reilly, John, Dec. 6, 1868.
O'Sullivan, R. J., Physician to Eastern Dispensary.
*Palmer, L. V.
Pardee, Charles J.
Parker, Willard, Prof. Surgery, College of Physicians and Surgeons.
*Parkinson, W. B., May 11, 1856.

* Deceased.

*Paul, James C., May 5, 1859.
Paine, Martyn, Prof. Mat. Med., etc., Med. Dep. N. Y. University.
Peaslee, Edmund R.
*Pennell, Rich., Apr. 11, 1861.
Peters, Geo. A., Surgeon to N. Y. Hospital.
*Phillips, Sam'l B., Mar. 3, 1857.
Pinkney, Howard.
*Piatt, Wm. F., May 6, 1848.
Pomeroy, O. D., Physician to Northern Dispensary.
Pond, Jas. O.
Pooley, James H.
Pooley, Thomas R.
*Porter, Mort. G., Nov. 24, 1863.
Post, A. C., Professor Surgery, Med. Dep. N. Y. University.
*Power, William, Sept. 14, 1858.
*Pratt, Peter, —— 1860.
Prince, C.
Purdy, Alfred S.
Purdy, James W.
Purdy, S. A.
Purple, S. S.
Putnam, F. A.
Ranney, H. D.
Ranney, J. W.
Ranney, L., Surgeon, Metropolitan Police.
*Ranney, M. H., Dec. 7, 1864.
Randolph, Israel.
Raphael, B. I., Surgeon, Jews' Hospital.
*Ray, Robert, Jr., July 3, 1860.
*Reese, David M., May 13, 1861.
Reynolds, James B.
Richards, T. W.
Roberts, Wm. C.
*Robeson, A. B., Mar. 22, 1853.
Rockwell, A. D.
Rodenstein, Louis A., Physician to N. Y. Institution for Deaf and Dumb.
*Rodgers, J. K., Nov. 9, 1851.
*Rogers, J. S., Mar. 29, 1851.
Rogers, Stephen.
Roosa, D. B. St. John, Aural Surgeon, N. Y. Eye and Ear Infirmary.
Rosenberg, Emil, Physician to German Dispensary.
Ross, James, Physician to N. Dispensary.
Sabine, Gustavus A., Consulting Physician to N. Y. Woman's Hospital.
Sands, A. L.
Satterlee, R. S., Brig.-General U.S.A.
Sayre, L. A., Prof. Surg., Bellevue Hosp. Med. Coll.
Schilling, E.
Schirmer, Wm.
*Schmidt, John W., Jr.
Sewall, John G.
Shanks, John.
*Sheppard, J. W.
*Sherwood, Burritt, Aug. 10, 1854.
Shrady, Geo. F.
Sims, J. Marion.
Sloan, W. J., U.S.A.
*Smith, David, Jan. 17, 1867.
*Smith, Gilbert, July 16, 1851.
Smith, G. M., Physician to Demilt Dispensary.
Smith, Hanbury.
Smith, James O.
Smith, Jerome C.
*Smith, Joseph M., Prof. Mat. Med., etc., Coll. Phys. and Surg., April 22, 1866.
Smith, J. Lewis, Physician to Charity Hospital.
Smith, Oscar G.
Smith, Stephen, Prof. Anat., Bellevue Hosp. Med. Coll.
*Snowden, John, Jan. 1848.
Speir, S. F.
Spreng, Justus J.

* Deceased.

*Spring, Edward, Feb. 13, 1850.
Squibb, E. R.
*Stearns, John, March 18, 1848.
*Stephenson, Mark, Aug. 28, '65.
Stephenson, Marcus P.
*Stevens, Alex. H., Prof. Emeritus Clin. Surg., Coll. Phys. and Surg., March 30, 1869.
*Stewart, James, Sept. 12, 1864.
*Stickney, J. Dwight, Sept. 30, 1849.
Stiles, R. Cresson, Consulting Physician, Kings Co. Hospital.
*Stimpson, E. B., May 15, 1858.
Stirling, Thomas B., Physician to Asylum for Lying-in Women.
Stone, John O.
Storer, Ebenezer.
Swan, Chas. Y.
*Sweeny, Hugh, September 15, 1857.
Sweeny, James.
*Swett, John A., Sept. 18, 1854.
Swift, Foster.
*Swift, H. S.
*Taft, Marcus L., Feb. 8, 1850.
Taylor, Charles F.
Taylor, Isaac E., Pres. and Prof. Obst., etc., Bellevue Hosp. Med. Coll.
Teats, Sylvester.
Teller, Seligman, Physician to Jews' Hospital.
Tellkampf, Theodore A.
*Thayer, Henry W., —— 1857.
Thebaud, J.
Thoms, W. F., Physician to N. Y. Dispensary.
Thomas, T. Gaillard, Prof. Obst., Coll. Physicians and Surgeons.
Thomson, W. H., Physician to Charity Hospital.
Thompson, George.
Townsend, John F.
*Townsend, Peter S., Mar. 26, 1849.
Tucker, C. P.
*Tucker, Geo. H., Jan. 25, 1863.
Tuttle, John T.
*Uhl, David, Sept. 17, 1858.
Underhill, Alfred, Physician to Orphans' Home, etc., Prot. Epis. Ch.
*Vaché, Alex. F., June 9, 1857.
Van Arsdale, Henry.
*Van Arsdale, Peter.
Van Buren, Wm. H., Prof. Anat. Med. Dept. N. Y. University.
Van Buren, P., Surgeon, Metropolitan Police.
*Van Buren, Thomas, ——1848.
Vanderpool, Edward.
Vandervoort, John L., Librarian, N. Y. Hospital.
Vanderveer, J. H.
Van Doren, M. D.
Van Kleek, John R.
Van Pelt, M. D.
Van Winkle, Ed. H.
Varick, Theodore R.
*Vedder, Joseph H., July 18, 1864.
Vermilye, Wm. E.
*Von Roth, Woldemar, — 1857.
Warner, E. B., Physician to N. Dispensary.
*Washington, J. H., Aug. 30, 1847.
*Watson, John, June 3, 1863.
*Watts, Robert, Prof. Anat., Coll. Phys. and Surg, Sept. 8, 1867.
Weber, Leonard.
Weeks, Cyrus.
Weir, Robert F.
Wells, Ovid P.
White, Francis V.
White, Ambrose L.
White, Oliver.
*White, Samuel P., June 6, 1867.
White, William T.
Whitehead, Wm. R.

* Deceased.

Wilkes, George, Consulting Surgeon, N. Y. Eye Infirmary.
Williams, M. W.
*Winchell, M. E., May 1, 1864.
Winston, Gustavus S.
*Wood, Isaac, Consulting Physician to Bellevue Hospital, March 25, 1868.
Wood, James R., Prof. Surg., Bellevue Hosp. Med. Coll.
Wood, Stephen.
Woodhull, Henry W. B.
*Woodward, G. F.
Worster, Joseph.
Wright, Charles.

NON-RESIDENT FELLOWS.

Francis E. Berger, Paris.
Charles A. Lee, Fishkill, N. Y.
F. Campbell Stewart.
*H. Van Arsdale, Morristown, N. J.
F. U. Johnston, Cooperstown, N. Y.
J. W. Corson.
J. C. Hepburn.
Nelson, L., North Aikin, S. C.

* Deceased.

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